AN

ELEMENTARY TREATISE

ON

GEOLOGY:

DETERMINING

FUNDAMENTAL POINTS IN THAT SCIENCE,

AND CONTAINING

AN EXAMINATION OF SOME

MODERN GEOLOGICAL SYSTEMS,

AND PARTICULARLY OF THE

HUTTONIAN THEORY OF THE EARTH.

BY

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TRANSLATED FROM THE FRENCH MANUSCRIPT,

BY THE

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TRANSLATOR'S PREFACE.

THE friendship, with which I am honoured by MR. DE LUC, induced him some time ago to communicate to me a Treatise on *Geology*, intended for the French Press. He was desirous that this work should appear in England, which he has for thirty-six years considered as his native country; and at his request, I ventured to undertake the following translation, which has been conducted and completed under his immediate inspection.

In revising his work, previously to its translation, some alterations occurred to the author as expedient. The French and English publications will therefore occasionally differ. The Letters also to DR. HUTTON, published

in the Monthly Review, are omitted in this work, though inserted in the original now printing at Paris.

To establish some fundamental points in Geology, is the principal object of Mr. De Luc; an object necessarily connecting almost every branch of the physical sciences, with the History of the Earth. This is clearly shown in the following pages, and the attention of the student is directed to the precise points of natural philosophy, and natural history, embraced by geology, and which have long excited a considerable interest.

Mr. De Luc has not yet been able to prepare for publication his earlier travels in Switzerland and in Germany, from the year 1782 to 1799. But he considers those which will shortly appear in English, containing his observations on some parts of the coasts of the Baltic and North Seas, and in different parts of England, as sufficient to establish all the propositions of the theory, the critical discussion of which forms the subject of the present work.

It has been justly observed that the discoveries in the physical sciences, of which recent times may boast, are to be ascribed to the influence of those rules of legitimate philosophy which were established by Lord Bacon two centuries ago. To these rules no philosopher has more scrupulously adhered than the excellent author of this work; and while he has thus largely contributed to rescue geological science from the disgrace of visionary theories, he has advanced a system which makes a ſtrong appeal for its truth to facts easily verified, if their certainty be disputed.

The origin and nature of Man form the ultimate objects of Mr. De Luc's inquiries. A knowledge of the History of man, he has observed, is indispensably necessary in order to investigate with success these most interesting subjects: and as his History is intimately connected with that of the planet which he inhabits, it becomes requisite to study with attention the monuments of the revolutions which the surface of the globe has undergone,

together

together with the causes which have produced them.

The results of Mr. De Luc's long and laborious researches have supplied an additional proof in support of revealed Religion. He has made it appear that the very facts upon which those systems seemed to rest, which assign an indefinite antiquity to our present continents, confirm, on the contrary, in the most satisfactory manner, the chronology of Genesis; and he has successfully traced the striking analogy that subsists between the great Volume of Nature, and the sacred page of Revelation. Thus he has shown, that the revolution which, according to every fact of geology, gave birth to our continents, was effected at the epoch of the Deluge, as it is described in the Mosaic History; and thus likewise he has demonstrated the conformity of geological monuments with the sublime account of that series of operations which took place during the *Six Days*, or periods of time, recorded by the inspired penman.

Mr.

Mr. De Luc's *Geological Letters to Professor Blumenbach*, in the British Critic for 1794 and 1795, were intended to point out those connexions: he seldom alludes to them in this work, the subject of which is purely philosophical: but the last of these Letters, on account of its very interesting nature, is, with the author's consent, reprinted here as an Appendix.

The numerous extracts from M. DE SAUSSURE'S *Voyages dans les Alpes*, adduced by Mr. De Luc in confirmation, either of his system, or of his personal observations, will form, it is presumed, a valuable part of this volume; and they will prove the more acceptable to the English reader, as no translation of the Travels of that eminent geologist into our language, has yet appeared.

HENRY DE LA FITE.

WHILE this work was in the press, Dr. HALES's first volume of his *New Analysis of Chronology, &c.* appeared; and on that publication Mr. De Luc has communicated to the translator the following remark:—" I regret that the Rev. Dr. Hales, " in his work just published, has deemed it necessary " to animadvert upon my Geological system, from " some misconceived notions of it; classing me among " the Geologists who have thought—That *the ante-" diluvian mountains did not exceed 50 toises in " height;*—And that *before the Deluge, the earth's " axis was perpendicular to the plane of the ecliptic, " and that its present obliquity took place at the " Deluge, which occasioned an alteration in the " earth's centre of gravity.* P. 329. If Dr. Hales " had read my works, or only the VIth of my Let-" ters to Professor Blumenbach, in the British " Critic of 1795, (which, as containing a general " view of the agreement of my system with Genesis, " is, in conformity to your wish, to be subjoined to " your translation) he would, in the first place, have " seen, that I never attempted to fix the height of " the antediluvian mountains, a point wholly indif-" ferent to my system. And with respect to changes " in the earth's motions, he would have perceived " that I considered those which I have mentioned, " not as *a cause* of the Deluge, but as *consequences* " which might have influenced the astronomical ob-

" servations

"servations subsequent to that event. On this "subject, Dr. Hales may see, in a review of Mr. "PHIL. HOWARD's work, *The Scriptural History* "*of the Earth, &c.* (British Critic, August 1797, "p. 108,) that these small changes are adduced as "accounting for the remarkable circumstance of the "antediluvian year consisting only of 360 days. "This circumstance served as the foundation of the "ancient astronomical formulæ, transmitted by "Noah to the Asiatic nations; formulæ which, "however accurate with regard to the antediluvian "state of the earth, had subsequently required cor-"rections, on account of a greater number of *days* "in the *year*, i. e. of *revolutions* of the earth on its "axis, while describing the same orbit."

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* In the original there is an accidental repetition of the numbers of some of the sections; a similar repetition has necessarily occurred in the translation, in order that the numbers may correspond.

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exist. 72. The object of the following Dissertation will be to examine how far the *original state* of each part of the surface of our continents can be determined by an investigation of the *causes* which have acted on them, and of the *effects* produced by those causes.

DISSERTATION

On the geological phenomena which determine *the state of our continents at their birth*. Preceded by some general remarks on the causes which retard decisions in the study of Nature; and containing an examination of Dr. HUTTON's *Theory of the Earth*,

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rival theories of *elevation* and of *subsidence* considered with a view to that phenomenon. 282, 283. Necessity, in the former, of supposing the blocks to have travelled in some manner upon the *surface;* since their ejection from within by the *expansible fluids, liberated* in that very act, must have been immediately followed by the *sinking* of the *raised continents.* 284. In the theory of *subsidence*, the expansible fluids contained in the *caverns* must have increased so much in *density* from the enormous *pressure* exercised on them by the *subsiding masses*, as to have rushed out through the intervals of those masses with an irresistible impetuosity, throwing up to the surface whatever fragments they encountered in their way. 285. The *lowest strata* of the masses having suffered the greatest collision, their fragments are found on the surface in the greatest abundance; those fragments varying in species, in a manner correspondent with what is known of the strata themselves. 286. Importance of local *lithologies*, for the purpose of improving our acquaintance with the internal parts of our continents. 287. The *conflict* produced between the *water* making way into the caverns during the descent of the great masses, and the *expansible fluids* then rushing out, was a *cause of the trituration and dispersion of the fragments on the bottom of the sea*, far more powerful than any *action of water* upon the *continents.* 288. Necessity of attentively considering the subject in every part. 289. Mr. Playfair invited to farther discussion. 290. REVIEW OF THE POINTS ESTABLISHED BY THE PRECEDING INVESTIGATIONS. 291. Of the data respecting events *previous* to the *birth of our continents*, contained in the foregoing conclusions. 292. The proofs of these propositions too much diversified to be detailed in this work. 293. They depend in part on *experimental philosophy* and *chemistry:* 294. And in part on the *monuments* remaining of the action of *past causes*, which monuments cannot be distinguished, without determining the *state of our continents at their birth.* 295. The great *geological questions* connected with the latter objects are brought forward generally in this work; and their particular *proofs* will be given in Mr. De Luc's Travels. 296. Common *successions of strata* in the *Alps.* 297. Their order different in different countries. 298. This subject requires to be farther illustrated by future observations.

299. All

299. All our knowledge of the *mineral strata* is derived from their *catastrophes*, by which masses of the inferior strata have been raised up, and thus brought to sight. 300. General importance of the subject. 301. Introduction to an appendix, containing remarks on some of Sir James Hall's experiments, and Mr. Kirwan's Geological Essays.

REMARKS ON SOME EXPERIMENTS OF SIR JAMES HALL.

§ 302. These experiments have been considered as a confirmation of Dr. Hutton's hypothesis respecting the *fixed air* in the *calcareous strata*, which he supposed to have been prevented by the *pressure of the ocean* from *escaping*, when subjected to a *consolidating heat*. 303—306. Short account of those experiments. 307, 308. Analogy between them, and some which have been made on *aqueous vapour*, in supposed opposition to Mr. De Luc's theory of *evaporation*. 309, 310. Mr. De Luc's reply to the latter. 311. Application of that reply to Sir J. Hall's experiments; showing the difference between the *resistance of a solid*, which in them was opposed to the production of the fixed air, and the *pressure of the ocean*, which that fluid would be able to pervade with the same facility, with which *aqueous vapour* pervades the *atmosphere*. 312. Could the inference drawn from these experiments be maintained, it would really be fatal to Dr. Hutton's theory; since it would prove that the *expansible fluid*, supposed, in that theory, to have elevated the whole mass of strata, could never have been *formed* under the pressure of that mass, in addition to that of the ocean.

REMARKS ON MR. KIRWAN'S GEOLOGICAL ESSAYS.

§ 313. Mr. Kirwan's object, as well as Mr. De Luc's, is to support the authority of *Genesis*, by confuting the systems in opposition to it. 314. Mr. Kirwan, though eminent in chemistry and mineralogy, has himself but little examined the various phenomena

phenomena of the earth, and has placed too great confidence in the descriptions of inaccurate observers. 315. His system is founded on his persuasion that our *present continents* are the *same* with those on which the *deluge* took place; but Mr. De Luc's is founded on the study of *geological monuments*, which evidently show that our continents formed the *bed of the sea*, till they became *dry land* in consequence of the revolution which produced the deluge. 316. Both set out from the period when the globe was encompassed by a *liquid;* the *precipitations* from which are supposed by Mr. De Luc to have been at first disposed in *strata;* but by Mr. Kirwan to have immediately formed the *primitive mountains*. 317. Both suppose the quantity of the liquid to have *diminished* on the surface; but Mr. Kirwan believes that the caverns into which it sunk had been produced by preceding *volcanoes*, and that, during the same period, *secondary mountains* were formed of materials detached from the *primitive*. 318. Impossibility that those who had examined these mountains could ascribe to them such a formation. 319, 320. Circumstances by which M. de Saussure was led to determine that the *strata*, which compose mountains, had at first been formed in a *horizontal* situation. 321. Mr. Kirwan agrees with Mr. de Luc that the *deluge* was produced by the waters of the *sea*. 322. The deluge was unquestionably a *miraculous* event; but the effects of whatever cause immediately produced it must have been consistent with the ordinary course of nature, and with the present appearance of the earth's surface. 323. Mr. De Luc's system shown to conform to these conditions. 324. In Mr. Kirwan's, the waters of the deluge are supposed to have proceeded from the *southern ocean*, to have rushed *northwards*, covering the tops of the highest mountains, and then to have *returned* with equal impetuosity, and passed again into the interior of the globe. 325. He ascribes the ruinous aspect of hills and mountains to this violent attack of the sea, though so evidently produced by catastrophes of a different kind; and he supposes the remains found in the northern hemisphere of animals, which now live only in the southern, to be a proof of a *sudden motion* of the sea from *south* to *north*. This affords no means of accounting for the fossil remains of marine animals, which now no longer exist in any sea, and are found chiefly in the *stony*

strata.

strata. Impossibility that the bodies of the larger quadrupeds could have floated over chains of mountains. 326. Mr. Kirwan ascribes to the deluge those vast accumulations of shells, which are found in *loose soils.* Description of the circumstances attending such accumulations: the deductions from them shown to be directly subversive of his hypothesis. 327. The effects of sudden percussion, and of the residence of a liquid mass on solid substances, require to be determined, before the outlines of our continents can be ascribed to the operations of the deluge, as supposed by Mr. Kirwan. 328. No precise objections adduced by Mr. Kirwan against Mr. De Luc's theory. 329. The conclusions drawn in the present volume from *general phenomena* might suffice for the establishment of fixed and permanent bases in geology. 330. But, in order to command conviction, many precise facts, of every *class* to which these phenomena belong, must be detailed. 331. Such details will shortly be given in Mr. De Luc's Travels.

ERRATA.

Page 11, . . . 4 fr. the bottom, *for* chimeral *read* chimerical
18, line 2 from the bottom, *for* conjesta *read* congesta
91, . . . 10 fr. the bottom, *for* ground *read* globe
96, . . . 2, *for* phenomena *read* phenomenon
103, bottom line, *for* as *read* at first
112, line 4 fr. the bottom, *for* disposition *read* deposition
201, *dele the inverted commas before* the
243, l. 4, *for* takes *read* take
266, l. 7, *for* water-trucks *read* water-tracks
273, l. 16, *for* remain *read* remains.

PRELIMINARY DISCOURSE

ON

GEOLOGY.

1. OF all the sciences, the most extensive and the most complex is that which was termed GEOLOGY, long before it was entitled to the name. This appellation was thus prematurely bestowed on the science, in consequence of the inconsiderate haste against which the immortal BACON repeatedly cautioned those who devoted themselves to the study of nature. The mind is at all times so eager to draw inferences, that it will not stop to collect all the data necessary for deducing legitimate conclusions, respecting the objects on which it is employed, but is not unfrequently rash in proportion to the importance of the subject, while this very circumstance ought to preclude all precipitate decisions. Hence arises a considerable obstruction to the real advancement of science, the progress of which is much less retarded by ignorance than by error.

2. No sooner had physical inquiries been so far pursued as to lead to the observation of phenomena,

which indicated a series of past effects, than causes were assigned to them from every quarter. This gave birth to a variety of Histories of the Globe, of Theories of the Earth, which differed as widely from each other as the fancies of their respective inventors, or as the particular phenomena which had led them to some general conclusion.

3. Yet if there be a science, in which advances ought to have been made with that scrupulous caution so judiciously recommended by the great master to whom I have just alluded, it is *geology*;—for the history of the earth is inseparably connected with that of man. The other speculative sciences are, for the most part, interesting only to those who cultivate them, and the errors which they may commit are of little consequence to the rest of mankind; but every man is greatly interested in the decisions which respect his abode: for if it can once be ascertained to have undergone various revolutions, some of these may have involved the human race, and on them may depend the solution of the question, *What is Man?*

4. It is here only that I shall permit myself to make an observation on this subject, in order to point out its importance; and this it is incumbent on me to do, to the end that they, whose precipitation I have blamed, may be more upon their guard, while they follow me in the path which I shall pursue. Geology was useless to men in a moral view, previously to the appearance of those numerous systems, which were hazarded in the course of the last century. To the

the Jews, the Christians, and the Mahometans, the book of *Genesis* presented the history of the earth, and of mankind; and if the Pagans were not favoured with such direct means of information, the grand outline of their mythologies bore so striking a resemblance to the Mosaic records, that it was impossible not to acknowledge them to be, in common with the other nations, the descendants of Noah; a circumstance which strongly confirmed the authority of Genesis to those who were so fortunate as to possess that exact history of the origin of the human race, and of the principal events which have befallen it.

5. No essential information, therefore, upon this subject, was wanting to men; nor indeed can it be conceived that such knowledge should have been withheld from them, if they be considered only as rational and moral, creatures of a God infinitely wise and good; for such a Being could not have suffered them to remain ignorant of his existence, and of the relations which they bore to Him. This reflection alone enabled the Theist to repel the arguments of a few sceptics against the reality of a revelation from God to man.

6. But a complete change was made in this state of things by geology. All those, who have formed geological systems, have endeavoured to rest them upon facts, which relate to the history of the earth. This has called back the attention of men to that history, as it is contained in Genesis, and consequently to that of the human race, with which it is connected;

and

and it was soon inferred, that, if geology were contrary to Genesis, the latter must be fabulous. All attempts to elude so evident a conclusion would prove vain; and it must of necessity be admitted, that if geology, a science founded on facts, and on strictly logical deductions, having attained all the characters of truth, were in reality found to be in opposition to what Genesis relates of those physical events of the earth, which are intimately connected with the human race, the history of the latter would become vague and uncertain. This is a consideration which I offer to those, whose profession it is to teach and defend revealed religion. The weapons by which it has been attacked have been changed, and our modes of defence must be adapted to the arms of its assailants. They now attack it by geology; which therefore becomes a science as essential to theologians, as the study of the learned languages, or of those ancient arguments, which are already much neglected in the present times, and which must henceforth derive their chief support from the very science, through the medium of which, under the pretence of an appeal to facts, it is attempted to set them aside.

7. In proportion, therefore, to the influence which geological systems will necessarily have, must be the caution observed by the friends of mankind in their inquiries respecting this great object. No general inference, indeed, drawn from the physical sciences, could be more important to men than that in which Genesis was involved; for to consider that book as fabulous was to plunge them into a final

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uncertainty, with regard to what it most concerned them to know, viz. their origin, their duties, and their destination: it was sapping the very foundation on which the great edifice of society has always rested: it was, in short, abandoning men to themselves; and those must have been little acquainted with them, who did not foresee the fatal consequences which would inevitably ensue.

8. Now the naturalists, who first published geological systems contrary to Genesis, were guilty of this want of consideration; for time has pronounced sentence on them; all these systems have already fallen, in consequence of the increase of knowledge; and thus is become obvious the imprudence of their attack on that, which a sublime tradition had established among men. Impelled by the love of fame, and solicitous for reputation, they lost sight of that future state, which constituted the chief value of their own existence, and were wholly inattentive to the rest of mankind. This is the most favourable interpretation of the conduct of those who have manifested, and still continue to manifest, such levity and imprudence in their geological pursuits.

9. I shall not apologize for those, who, without possessing a greater share of information, have treated the same subject with a view of defending Genesis. For this premature undertaking has likewise produced various chimerical systems; and their authors did not consider that, as the science of which they ventured to treat, was of vast extent, they could

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not flatter themselves that their researches had been sufficiently deep to guard them against the danger of being contradicted by new facts, and of thus injuring the cause, which it was their object to defend. And so, in reality, it has happened; for the adversaries of Genesis, by overthrowing those groundless systems, imagined that they triumphed over Genesis itself. While geology, therefore, was in its infancy, philosophers should have contented themselves with pointing out the errors of the anti-mosaical systems, for which the state of the physical sciences was sufficiently advanced to have readily furnished them with the means; and they should not have hazarded theories, before they had taken every requisite precaution to fix them upon an immoveable foundation.

10. I shall no longer dwell upon the connection subsisting between geology and Genesis; a connection which I have pointed out in other works. In adverting to it in this place, I have been influenced by no other motive, than the obligation to set forth the importance of the subject; and I solicit the attention, not only of those who themselves endeavour to form systems, but of that more numerous class, consisting of those who read them, and wish to form fixed opinions. This is not merely one of those subjects of natural history, or natural philosophy, in regard to which it would be of little consequence to mankind, whether erroneous theories were formed or adopted: a most important moral object is combined with it, by the indissoluble connection which subsists between the history of the human race, and that of the earth; an object, which ought to be always

always kept in view, as well by those, who, not pursuing the road which alone can lead to truth, may betray mankind into fatal error, as by those, who, not having qualified themselves by previous study to judge of these systems, embrace hypotheses from mere appearances. I more particularly request the latter to suspend their judgment in regard to geology, until they shall have bestowed upon it sufficient time to enable themselves to become at least acquainted with its extent; and I as earnestly entreat those, who have formed or adopted systems which I shall hereafter combat, to follow me with caution in the path which I shall tread, while I establish some fundamental points in geology; to do which will henceforth be my only object.

11. It will be proper, in this place, to mention a theory, to which I shall frequently have occasion to advert in the course of my discussions. Mr. John Playfair, Member of the Royal Society of Edinburgh, and Mathematical Professor in that University, after the decease of his friend Dr. Hutton, undertook the defence of his Geological Theory, and republished it at Edinburgh, in the year 1802, under the title of *Illustrations of the Huttonian Theory of the Earth.* This is one of the latest and most methodical works on geology; written by a man possessed of considerable talents; who sets out from general data no less true than they are important; who appears invariably to proceed with the most guarded caution, and knows how to diffuse a peculiar interest upon the discussion of scientific subjects. In

In order to give some general idea of the subject of this work, which I shall have principally in view, I shall begin by transcribing the Advertisement.

"The Treatise here offered to the public, was "drawn up with a view of explaining Dr. Hutton's "Theory of the Earth in a manner more popular "and perspicuous than is done in his own writings. "The obscurity of these has often been complained "of; and thence, no doubt, it has arisen that so "little attention has been paid to the ingenious and "original speculations which they contain.

"The simplest way of accomplishing the object "proposed, seemed to be, to present a general out-"line of the system, in one continued Discourse; "and to introduce afterwards, in the form of notes, "what farther elucidation any particular subject "was thought to demand. Through the whole, I "have aimed at little more than a clear exposition "of facts, and a plain deduction of the conclusions "grounded on them; nor shall I claim any merit "to myself, if, in the order which I have found it "necessary to adopt, some arguments may have "taken a new form, and some additions may have "been made to a system naturally rich in the num-"ber and variety of its illustrations.

"Of the qualifications which this undertaking "requires, there is one that I may safely suppose "myself to possess. Having been instructed by "Dr. Hutton himself in his Theory of the Earth;

"having

" having lived in intimate friendship with that ex-
" cellent man for several years, and almost in the
" daily habit of discussing the questions here treated
" of; I have had the best opportunity of under-
" standing his views, and becoming acquainted with
" his peculiarities, whether of expression, or of
" thought. In the other qualifications necessary
" for the illustration of a system so extensive and
" various, I am abundantly sensible of my defi-
" ciency, and shall therefore, with great deference,
" and considerable anxiety, wait that *decision* from
" which there is no appeal."

12. By such a *decision* ought to be understood that of facts; and I shall adduce no other arguments upon the objects in regard to which I do not acquiesce in the theory which Mr. Playfair has attempted to defend. From the time that this theory first appeared, I bestowed upon it all the attention, which the genius of the author so justly claimed, and to which his politeness in sending me a copy of it, on its publication in the Philosophical Transactions of the Royal Society of Edinburgh, was entitled. I saw him gradually drawn into the adoption of a general system, in consequence of some mineralogical details, on which his imagination had strongly dwelt, without having sufficiently noticed a number of important phenomena exhibited on the surface of the earth. His field of observation was confined to the island of Great Britain, and more particularly to Scotland, where every thing appeared to him to con-

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10. I shall no longer dwell upon the connection subsisting between geology and Genesis; a connection which I have pointed out in other works. In adverting to it in this place, I have been influenced by no other motive, than the obligation to set forth the importance of the subject; and I solicit the attention, not only of those who themselves endeavour to form systems, but of that more numerous class, consisting of those who read them, and wish to form fixed opinions. This is not merely one of these subjects of natural history, or natural philosophy, in regard to which it would be of little consequence to mankind, whether erroneous theories were formed or adopted: a most important moral object is combined with it, by the indissoluble connection which subsists between the history of the human race, and that of the earth; an object, which ought to be

always

always kept in view, as well by those, who, not pursuing the road which alone can lead to truth, may betray mankind into fatal error, as by those, who, not having qualified themselves by previous study to judge of these systems, embrace hypotheses from mere appearances. I more particularly request the latter to suspend their judgment in regard to geology, until they shall have bestowed upon it sufficient time to enable themselves to become at least acquainted with its extent; and I as earnestly entreat those, who have formed or adopted systems which I shall hereafter combat, to follow me with caution in the path which I shall tread, while I establish some fundamental points in geology; to do which will henceforth be my only object.

11. It will be proper, in this place, to mention a theory, to which I shall frequently have occasion to advert in the course of my discussions. Mr. JOHN PLAYFAIR, Member of the Royal Society of Edinburgh, and Mathematical Professor in that University, after the decease of his friend Dr. HUTTON, undertook the defence of his Geological Theory, and republished it at Edinburgh, in the year 1802, under the title of *Illustrations of the Huttonian Theory of the Earth.* This is one of the latest and most methodical works on geology; written by a man possessed of considerable talents; who sets out from general data no less true than they are important; who appears invariably to proceed with the most guarded caution, and knows how to diffuse a peculiar interest upon the discussion of scientific subjects.

In

In order to give some general idea of the subject of this work, which I shall have principally in view, I shall begin by transcribing the Advertisement.

"The Treatise here offered to the public, was "drawn up with a view of explaining Dr. Hutton's "Theory of the Earth in a manner more popular "and perspicuous than is done in his own writings, "The obscurity of these has often been complained "of; and thence, no doubt, it has arisen that so "little attention has been paid to the ingenious and "original speculations which they contain.

"The simplest way of accomplishing the object "proposed, seemed to be, to present a general out-"line of the system, in one continued Discourse; "and to introduce afterwards, in the form of notes, "what farther elucidation any particular subject "was thought to demand. Through the whole, I "have aimed at little more than a clear exposition "of facts, and a plain deduction of the conclusions "grounded on them; nor shall I claim any merit "to myself, if, in the order which I have found it "necessary to adopt, some arguments may have "taken a new form, and some additions may have "been made to a system naturally rich in the num-"ber and variety of its illustrations.

"Of the qualifications which this undertaking "requires, there is one that I may safely suppose "myself to possess. Having been instructed by "Dr. Hutton himself in his Theory of the Earth;

"having

" having lived in intimate friendship with that ex-
" cellent man for several years, and almost in the
" daily habit of discussing the questions here treated
" of; I have had the best opportunity of under-
" standing his views, and becoming acquainted with
" his peculiarities, whether of expression, or of
" thought. In the other qualifications necessary
" for the illustration of a system so extensive and
" various, I am abundantly sensible of my defi-
" ciency, and shall therefore, with great deference,
" and considerable anxiety, wait that *decision* from
" which there is no appeal."

12. By such a *decision* ought to be understood that of facts; and I shall adduce no other arguments upon the objects in regard to which I do not acquiesce in the theory which Mr. Playfair has attempted to defend. From the time that this theory first appeared, I bestowed upon it all the attention, which the genius of the author so justly claimed, and to which his politeness in sending me a copy of it, on its publication in the Philosophical Transactions of the Royal Society of Edinburgh, was entitled. I saw him gradually drawn into the adoption of a general system, in consequence of some mineralogical details, on which his imagination had strongly dwelt, without having sufficiently noticed a number of important phenomena exhibited on the surface of the earth. His field of observation was confined to the island of Great Britain, and more particularly to Scotland, where every thing appeared to him to con-

firm

firm his preconceived opinions; but the ideas to which particular geological objects really lead, can be understood only when they have been viewed under a variety of aspects, and in different combinations; and this renders it necessary to observe them in a great number of different countries. I considered Dr. Hutton's hypothesis as entitled to a regular examination on my part, accompanied by the results of my numerous observations; and I wrote to him four successive letters, which were inserted in the *Monthly Review* for 1790 and 1791. He published in 1795 a new exposition of his Theory, in two volumes; in this work he dwelt but little upon the subject of my letters, probably because the facts which I submitted to him were in the form of generalization only; he principally reverted to my first geological work, *Lettres sur l'Histoire de la Terre et de l'Homme*, written at a period when essential information was yet wanting on many parts of this subject, with which, however, succeeding years had furnished me when I wrote to him.

13. Mr. Playfair, who has followed his friend in the exposition of his theory, has given a very clear view of the great extent of knowledge which that important subject requires; and as this is a point necessary to be established, I shall first present a general idea of it in Mr. Playfair's own words. After having pointed out unequivocal marks of the great revolutions which the surface of the globe has undergone, he thus proceeds, p. 2.

"To

"To trace the series of these revolutions, to ex-
"plain their causes, and thus to connect together
"all the indications of change that are found in the
"mineral kingdom, is the proper object of a theory
"of the earth. But though the attention of men
"may be turned to the theory of the earth by a very
"superficial acquaintance with the phenomena of
"geology, the formation of such a theory requires
"an accurate and extensive examination of those
"phenomena, and is inconsistent with any but a
"very advanced state of the physical sciences.
"There is, perhaps, in those sciences, no research
"more arduous than this; none certainly where the
"subject is so complex, where the appearances are
"so extremely diversified, or so widely scattered,
"and where the causes that have operated are so
"remote from the sphere of ordinary observation.
"Hence the attempts to form a theory of the earth
"are of very modern origin; and as, from the sim-
"plicity of its subject, astronomy is the eldest, so,
"on account of the complexness of its subject,
"geology is the youngest of the sciences."

14. These observations are very just, inasmuch as they point out the vast field which was to be explored, before a theory of the earth could be established upon a solid basis. Is it surprising, therefore, that they, who had so hastily engaged in such pursuits, should have produced the most groundless and chimeral systems? One of the great inconveniences, which, in the conflict of theories thus hazarded, has resulted from precipitation, is the opinion entertained by many

many that these researches are vain; that it is impossible to trace back the present state of the earth to the causes which produced that state; and that philosophers should confine themselves to the investigation of those which are actually operating. This opinion, which deprives geology of the assistance of several men well qualified to facilitate its progress, prejudices against those results of the science, which are already clearly ascertained, many others, to whom they would be necessary to settle their ideas respecting the history of mankind. I shall again employ Mr. Playfair's expressions, at § 445 of his work, in order to describe this scepticism with regard to geology.

"Among the prejudices which a new theory of" "the earth has to overcome, is an opinion held, or" "affected to be held, by many, that geological sci-" "ence is not yet ripe for such elevated and difficult" "speculations. They would, therefore, get rid of" "these speculations, by *moving the previous ques-*" "*tion*, and declaring that at present we ought to have" "no theory at all. We are not yet, they alledge," "sufficiently acquainted with the phenomena of" "geology; the subject is so various and extensive," "that our knowledge of it must, for a long time, per-" "haps for ever, remain extremely imperfect. And" "hence it is, that the theories hitherto proposed have" "succeeded one another with so great rapidity, hardly" "any of them having been able to last longer than" "the discovery of a new fact, or a fact unknown when" "it was invented. It has proved insufficient to con-

" nect this fact with the phenomena already known, " and has therefore been justly abandoned. In this " manner, they say, have passed away the theories of " Woodward, Burnet, Whiston, and even of Buffon; " and so will pass, in their turn, those of Hutton and " Werner." He does not add that of De Luc, but others have done so upon the same grounds.

15. Some valuable remarks are contained in the arguments which Mr. Playfair adduces in opposition to this prejudice, and motives for hope are suggested by him, which indicate great penetration: I shall therefore again use his own words for expressing these motives. But, among the means which he points out to ensure success, I shall notice one, as it occurs in the course of the quotation, which will hereafter become an important subject of discussion.

" This unfavourable view of geology," he continues, " ought not, however, to be received without " examination; in science, presumption is less hurtful than despair, and inactivity is more dangerous " than error. One reason of the rapid succession " of geological theories, is the mistake that has been " made as to their object, and the folly of attempting " to explain by them the *first origin of things*. This " mistake has led to fanciful speculations, that had " nothing but their novelty to recommend them, and " which, when that charm had ceased, were rejected " as mere suppositions, incapable of proof. But if " it is once settled that a theory of the earth ought " to have no other aim than to *discover the laws that*

" *regulate*

"*regulate the changes on the surface, or in the in-*
"*terior of the globe*, the subject is brought within
" the sphere either of observation, or analogy; and
" there is no reason to suppose that man, who has
" numbered the stars, and measured their forces,
" shall ultimately prove unequal to this investiga-
" tion."

16. This is the means of success, in regard to which I differ from Mr. Playfair: he would lay down, as a fixed point in geology, the very part of his friend's theory, to which is probably owing the little attention which has, as he complains, been paid to it; namely, that nothing is to be the object of investigation in the history of the earth, but the laws that regulate the changes of its surface; excluding thus every inquiry respecting a primordial constitution of the earth, whence these changes might follow. It is not from its obscurity, as Mr. Playfair imagines, that his friend's theory has not attracted much attention, since it is not difficult to be understood; but its nature, which I shall hereafter discuss. I shall at present, therefore, confine myself to noticing a mistake, which he has himself committed in respect to other theories. I am acquainted with no geological system worthy of regard, attempting to explain *the first origin of things*, which in truth would be a folly. But, by tracing with the necessary attention the characters of geological monuments, a *succession* is clearly discernible, by means of which I believe it possible to go back to some epoch, at which none of them did as yet exist, and to determine what was then the state

of

of things upon our globe, and what are the causes from which its actual state has proceeded. For the present, I shall content myself with thus noticing those objects of discussion, of which hereafter I shall treat at large; and I now return to the arguments of Mr. Playfair against scepticism in geology, in which we fully agree.

" Again," he says, " theories that have a rational " object, though they be false or imperfect in their " principles, are for the most part approximations " to the truth, suited to the information at the " time when they were proposed. They are steps, " therefore, in the advancement of knowledge, and " are terms of a series that must end when the real " laws of nature are discovered. It is, on this " account, rash to conclude, that, in the revolutions " of science, what has happened must continue to " happen, and because systems have changed rapidly " in time past, that they must necessarily do so in " time to come.

" He who would have reasoned so, and who had " seen the ancient physical systems, at first all rivals " to one another, and then swallowed up by the " Aristotelian; the Aristotelian physics giving way " to those of Des Cartes, and the physics of Des " Cartes to those of Newton; would have predicted " that these last were also, in their turn, to give " place to the philosophy of some later period. " This is, however, a conclusion that hardly any one " will now be bold enough to maintain, after a hun-

" dred

" dred years of the most scrupulous examination " have done nothing but add to the evidence of the " *Newtonian system.* It seems certain, therefore, " that the rise and fall of theories in times past, does " not argue that the same will happen in the time " that is to come.

" The multifarious and extremely diversified ob- " ject of geological researches does, no doubt, ren- " der the first steps difficult, and may very well " account for the instability hitherto observed in " such theories; but the very same thing gives rea- " son for expecting a very high degree of certainty " to be ultimately attained in these inquiries.

" Where the phenomena are few and simple, there " may be several different theories that will explain " them in a manner equally satisfactory; and in " such cases, the true and the false hypotheses are " not easily distinguished from one another. When, " on the other hand, the phenomena are greatly " varied, the probability is, that among them some " of those *instantiæ crucis,*" (phenomena described by Bacon) " will be found, that exclude every hypo- " thesis but one, and reduce the explanation given " to the highest degree of certainty.—Hence the " number, the variety, and even the complication of " facts, contribute ultimately to separate truth from " falsehood; and the same causes, which, in any " case, render the first attempts toward a theory " difficult, make the final success of such attempts " just so much the more probable."

17. This

17. This new passage of Mr. Playfair convinced me of what I have already stated, that he had formed a very correct judgment of geology; and that, while he had seen the extent of the objects which it ought to comprise, he had not given way to the opinion that it exceeded the reach of human capacity. This passage, as well as many other parts of his work, make it evident that he has carefully studied the immortal Bacon, whose luminous principles are fallen at this day into so much neglect, to the great detriment of natural science. Mr. Playfair well understood that the voluminous writings of that great philosopher had not, for their ultimate object, what superficial naturalists would esteem as solely useful; namely, details of natural history and natural philosophy, to which such persons exclusively attend. Thus, judging Bacon in this single view, they consider themselves as much more enlightened than he was, although so eminent in his time; and suffer his volumes to remain upon the dusty shelves of libraries, without having perceived in them that, which will ever bring them to the recollection of true philosophers, and which Mr. Playfair has in many respects well understood, though he has sometimes departed from it.

18. The sagacity of Bacon enabled him to discover the immense variety of the phenomena of nature; the strength of his genius, however, prevented him from considering this variety as forming insuperable obstacles to the advancement of science, provided they were studied with less of the impatience and presumption, which, till then, had prevailed among phi-

 losophers.

losophers. In his *cogitata* prefixed to the *de Augmentis Scientiarum*, after having examined, with profound attention, the amount of human knowledge in his time, and the means by which it had been acquired, he thus proceeds: "Whence it "arises that these first notions of things, which "constitute the whole of the means employed by "human reason in the study of nature, are neither "well arranged, nor well built up, but form, as it "were, a massy fabric, magnificent indeed, but "without foundation *."

These few words indicate the rule by which Bacon was guided in his attempts, not to erect the edifice of science, for that requires the whole collection of facts, and he found none that were not ill chosen, and as ill-arranged; but only to lay the foundation of that fabric, by fixing principles of inquiry concerning the materials and their respective uses, and by furnishing the most precise instructions for ascending, by the determination of the true characters of phenomena, to the causes from which they originate.

19. It was only for the purpose of illustrating his precepts, that Bacon endeavoured to avail himself of what was in his time the least defective in the descriptions of phenomena afforded by natural history, and natural philosophy; and if we reflect on the collection

* Ex quo fit ut universa ista ratio humana, (notiones rerum primæ) qua utimur quoad inquisitionem naturæ, non benè conjesta et ædificata sit, sed tanquam moles aliqua magnifica sine fundamento.

of

of facts then assembled, it will in every respect be found very inferior to the stock of knowledge of the present day. But he was so far from considering the materials, which he had employed in those applications of his rules, as proper to remain, at least in the same state, in the edifice of science, that, after having pointed out in his great work, *De Augmentis Scientiarum*, the road which must be pursued, and the caution which would be necessary, in our endeavours to obtain true materials, he makes the following remark in the last chapter, lest the task should be supposed an easy one: "I recollect the saying of Themistocles; who "when he heard the deputy of an insignificant town "declaiming in a very pompous style, 'Friend,' "cried he, 'this discourse of thine requires a city.' "It may certainly, I am aware, be in like manner "objected to me, that my discourses require an age; "an entire age, perhaps, even for their trial, but "many ages for their accomplishment *."

20. Yet he was so far from considering the magnitude of the object which he had traced out, as a reason for despondency, or as an impediment to final success, that from this very circumstance, on the contrary,

* Interim in mentem mihi venit responsum illud Themistoclis, qui, cum ex oppido parvo legatus quidam magna nonnulla perorasset, hominem perstrinxit; Amice, verba tua civitatem desiderant. Certè objici mihi rectissimè posse existimo, quod verba mea seculum desiderent: seculum fortè integrum ad probandum; complura autem secula ad perficiendum. De Dign. et Augm. Scient. Lib. ix. p. 254. Vol. IV. of the London 4to. edition. 1765.

trary, he drew incitements to confidence and hope, provided that vain metaphysical speculations were abandoned, and the attention exclusively directed to facts, by pursuing the paths indicated by the phenomena themselves. He more particularly observes upon this subject, in the 112th Aphor. of the *Novum Organum*, "Let no man be alarmed at the multi-"tude of the objects presented to his attention, for "it is this, on the contrary, which ought rather to "awaken hope. . . . . If there were any one "amongst us, who, when interrogated respecting "the objects of nature, was always prepared to "answer by *facts*, the discovery of causes, and the "foundation of all sciences, would be the work of "few years *."

21. From Mr. Playfair's adoption of this idea, I am convinced that he has fully understood the great services rendered by Bacon to natural science; and he applies it to geology, when, in answer to those who consider it as a science beyond the reach of human capacity, in consequence of the multitude of objects which it should embrace, he observes, that "the same causes, which, in any case, render the "first attempts toward a theory difficult, make the "final success of such attempts just so much the "more probable." But there are many conditions

* Interim particularium multitudinem nemo reformidet, quin potiùs hoc ipsum ad spem revocet. . . . . . . Apud nos vero si esset præsto quispiam qui de facto naturæ ad interrogata responderet, paucorum annorum esset inventio causarum et scientiarum omnium. Nov. Org. Aphor. CXII. Vol. IV. p. 297.

attached

attached to this success; genius alone is not sufficient; and to ensure it, philosophers, as Bacon observed, must enable themselves to solve every *question* by *facts*. Now the more these questions are subdivided into branches, which, however remote from each other, still proceed from the same stem, the more numerous must be the classes of facts which it is necessary to embrace; and if, in particular spots, the phenomena are complex, by reason of their intermixture, our researches must be extended to others, where we may observe them either in their more simple state, or in different combinations, and this may serve to determine their essential characters; a condition, which will oblige us to seek for the same phenomena in many different countries, and to fix also our attention on those which other observers may have pointed out. Lastly, although, generally speaking, it is true, that, in all sciences of facts, theories, even false or imperfect in their principles, may promote their advancement, by leading to new researches, yet such is the character which geology has attained, by reason of its great importance to men, whose peace may be disturbed by false decisions, that every true friend of mankind ought to make the greatest exertions, to avoid every error on so important a subject. I have felt the necessity of that caution in following this career; and when *facts* have been opposed to my opinions, I have never failed to suspend my judgment, until I have verified them. This will be seen in the course of my travels, and particularly with regard to the facts quoted by Mr. Playfair.

22. Dr.

22. Dr. Hutton's Theory of the Earth, so much admired by his friend, is little known upon the continent; I shall therefore proceed to exhibit its outlines. I could not make choice of one, that would furnish scope for more essential discussions in geology; and discussions, on controverted points, afford the most efficacious means of fixing the attention upon the objects themselves. I have already remarked that this theory, though distinguished from every other by its own fundamental hypotheses, is however connected with hypotheses common to most of the theories which I have formerly opposed; but they are reproduced under different forms, and maintained with such method, with such an appearance of advancing only on the support of facts, that nothing can render more evident the necessity of determining these last with precision, than the discussions to which I shall be led. I shall therefore begin by stating the theory.

23. We have already seen Mr. Playfair expressing a wish, that, in order to prevent all fluctuation in geology, it should be laid down as an invariable rule, "that a theory of the earth ought to have no other "aim than to discover *the laws that regulate the* "*changes on the surface, or in the interior of the* "*globe.*" With regard to the general object, as well as to several parts of the theory, Dr. Hutton, and with him Mr. Playfair, frequently ground themselves upon *final causes:* the establishment of causes, productive of the "*changes*" which constitute the following theory, appears to them to manifest the most admirable

admirable ends, which ends, in their turn, serve to prove the reality of those "changes."

24. According to that theory, our globe is so constituted, that, while existing continents are destroyed by the action of air, of gravity, and of running waters, their materials, propelled by the latter to the coasts, are scattered, by the different actions of the sea, over the whole of its bed. A considerable internal heat indurates these materials, from whence result mineral strata, similar to those of which the mass of our continents is composed. When the existing continents have been destroyed by degradation, the same heat, which has consolidated the new strata at the bottom of the sea, forces them up; the sea is thus driven upon the abraded continents, and new continents are produced, exposed, in like manner, to the action of the air, of gravity, and of running waters; and their materials, by a repetition of the former process, are in their turn spread over the bottom of the ocean, where heat prepares new strata, which are themselves in due time lifted up, to form new continents.

25. Such are the hypotheses which characterise the Huttonian theory; these are the "*laws*," which it assigns as regulating the "*changes*" above-mentioned, and to the investigation of which Mr. Playfair would confine all future researches, without considering any *origin*. It is supposed that this alternate process of the destruction of continents, and of the formation of others with their materials, has been already many

times

times repeated. Each process may require millions of years; for the time which has elapsed since the birth of our continents is to be reckoned on such a scale; Dr. Hutton allows it; but this is no objection with him, since the past has no limits. He has not even attempted to give an idea of the number of times that this change has been effected; he found, no doubt, the task too difficult; but he and Mr. Playfair conceive it impossible to go back to a *primitive state* of the earth, in which nothing that is observed as the *effect of physical causes* had yet been operated.

26. In order to induce his readers to admit, without reluctance, these strange hypotheses, Mr. Playfair, to whom they are familiar, presents them under an aspect, which he acknowledges to be at first unfavourable. "Yet," (he says, § 133,) "with all "these circumstances of originality, grandeur, and "simplicity, in its favour, with the addition of evi-"dence as demonstrative as the nature of the subject "will admit, this theory has probably many obsta-"cles to overcome, before it meet the general appro-"bation. The greatness of the objects, which it "sets before us, alarms the imagination; the powers, "which it supposes to be lodged in the subterra-"neous regions; a heat, which has subdued the "most refractory rocks, and has melted beds of "marble and quartz; an expansive force, which has "folded up or broken the strata, and raised whole "continents from the bottom of the sea; these are "things, with which, *however certainly they may be* "*proved*, the mind cannot soon be familiarized."

26. *(a.)* Those who have not made geology their particular study, when they see Mr. Playfair insisting so positively upon demonstrative evidence in this theory, may be fearful of incurring the imputation of weakness, if they do not endeavour to "familiarize" themselves with the "greatness" of the objects which it presents: should they not, however, succeed in comprehending the force of this "evidence," they will either mistrust their own judgment, or conceive it at least to be sufficiently proved, that, in geological researches, we must content ourselves with seeking for the "*laws*" of certain "*changes*," without attempting to ascend to any *origin*; an endeavour, which Mr. Playfair, evidently mistaking its object, terms "the folly of attempting to explain the *first* "*origin of things.*"

27. Such is the theory which I propose to have in view; and although some readers, not surprised at what Mr. Playfair himself says, that little attention has been paid to that theory, may at first be of opinion that I might have chosen a more useful text, they will, I trust, be soon convinced that there exists probably no other system, which could have given rise to so many precise questions, important to geology. That theory, however, has been attacked on many sides, and I have opposed it myself in my letters to Dr. Hutton. Mr. Playfair had these attacks before him, though he has but seldom mentioned them; making it his principal object to present his friend's theory under such aspects, as would best guard it against them, and to adduce new proofs

in

in its support. This mode of defence will lead to many interesting discussions, the subjects of which I shall here sketch, as affording a general view of the several parts of geological science.

28. One of the greatest objects of this science, that which has required the most profound researches in every branch of natural philosophy, is the *origin of the mineral strata*, of which the whole mass of our continents is composed. There have been many hypotheses upon this subject, but, having originated in the infancy of science, they have been successively abandoned; and, with the exception of that which I am going to examine, the theory, which has been adopted by the most celebrated geologists, is, that those substances have been successively separated from a liquid by *chemical precipitations*. Dr. Hutton and Mr. Playfair think that they can demonstrate the impossibility of such an origin; and thus a wide field is opened to discussion, by the different objects which it embraces, all of importance in geology: these I shall now proceed to trace out.

29. In order to rest his own hypothesis respecting the origin of our mineral strata upon some facts, Dr. Hutton has undertaken to prove that our continents have already undergone a very great diminution, owing to the action of external causes upon them, and that the materials, which they have lost, have been scattered over the bed of the sea. His proofs, supported by Mr. Playfair, consist in supposing, with many other geologists, that the vallies among

mountains, and the dales between hills, are cavities produced by running waters, which have even much diminished the height of these eminences; and that the sea itself contributes to destroy the continents by attacking their shores. It is high time that this great geological question, which equally belongs to all the theories of the earth, should at length be decided, since every thing that relates to it is immediately exposed to view; and I shall make it appear, from actual observation, that, on the contrary, *the cavities of vallies and dales, and the abrupt cliffs of our coasts, are original characteristics of our continents*, and that the action of external causes, so far from having produced these characters, has in fact a direct tendency to efface them.

30. But a complete degradation of our continents, by which, in Dr. Hutton's opinion, they would at length be worn down to the level of the sea, could by no means answer the purpose of his theory: the diffusion of their materials over the whole of its bed would also be necessary, as he and Mr. Playfair, in common with other geologists, actually suppose it. Here then is a second object of great importance to geology, the decision of which likewise depends on facts; and from them I will shew that the *sediments deposited by rivers at their mouths, and the materials detached by the action of the sea from those shores, which were originally steep, and against which it beats, are all accumulated along the coasts.*

31. A third question, connected with the same subject, is the following: granting the possibility of our continents being entirely destroyed, and their materials scattered over the whole bed of the sea, would the final result of such a process be in any degree analogous to our mineral strata? This question, although Dr. Hutton's theory gives rise to it, regards likewise other theories, and is connected with the important object of the strata, which compose the whole observable mass of our continents: we are hence led to define them; and, by a comparative examination of their nature with that of the detritus of our continents, I shall prove that it is impossible to attribute such an origin to those strata.

Lastly, since the strata of which a great part of the mass of our continents consists are *stony*, a fourth question arises, viz. how could such consolidated strata have been produced from the materials of other continents, those materials having consisted only of pulverised matter and gravel? This question again is not foreign from other theories of the earth.

31. *(a.)* It would be tedious to specify, under each head, the various theories to which it relates; and it is sufficient to fix these points in geology itself, in order to set aside those systems which contain errors respecting them. And here the Huttonian theory has an advantage over the others, inasmuch as it contains a cause of the consolidation of those loose and incoherent materials. Dr. Hutton especially maintains

maintains that solid strata cannot be explained by any chemical cause; and that indeed there can be no other than that which he assigns, viz. the fusion of the materials by an excessive heat, acting at the bottom of the sea, and belonging to the original constitution of the globe. Now, on the contrary, I shall prove that *our strata have not been consolidated by the heat of fusion;* that no difficulty exists on this subject in the theory of chemical precipitation; and that his objections proceed only from his not having considered this theory under its true point of view.

32. After these questions, which relate only to the origin of our mineral strata, the next, no less important in geology, is, how has it happened that these strata, which have evidently been produced at th bottom of the sea, are at present above its level? In the infancy of geology, various hypotheses were formed respecting the production of our continents by the action of slow causes; but when I examined these hypotheses in my first geological work, I proved specifically of each of them, and generally of every other which might be formed with the same view, that they could proceed only from erroneous observations. These hypotheses have fallen to the ground, and all well-informed and attentive geologists concur in the general idea, that *the birth of our continents has been the effect of some great revolution upon our globe.*

33. The circumstance, which has more particularly led to the adoption of this idea, is the ruinous appearance

appearance observed in the whole mass of our mineral strata; a circumstance daily more and more manifested, by the fractures discovered throughout the whole thickness of this mass, by the changes in the relative level of their parts, which, however, must have been originally continuous, and by the angular movements evidently undergone by those strata, which, having certainly been produced in a horizontal situation, are yet considerably inclined in every direction, and even vertical in many masses constituting mountains. There no longer exists any doubt respecting these great symptoms of convulsions in the terraqueous globe; a point which naturally connects itself with the idea that the birth of our continents is to be assigned to some great convulsion of that nature.

34. This, I repeat, is a point on which the most eminent geologists agree; but they are divided with respect to the connexion between the catastrophes, of the strata, and the birth of our continents. And here we have the following dilemma: since these strata have indisputably been formed at the bottom of the sea, but yet are at present above its level, either their mass has been raised, or the level of the sea has sunk. Many geologists, and among others Dr. Hutton, have adopted the opinion that the continents have been raised, and that it was during this process that our strata underwent the catastrophes, so strongly impressed throughout their mass: while the other proposition of the dilemma, namely, that the level of the sea has sunk, constitutes a part

of my system, with this essential circumstance, that the catastrophes indicated by our strata successively happened, while they yet formed the bed of the sea. There, then, and before their birth, our continents acquired the form of their present surface; and *the event of their birth was produced by the subsiding of other continents, in consequence of which the sea, abandoning its original bed, was transported to a different part of the globe;* these different catastrophes being, however, effects of the same cause. This is one of the greatest objects of geology; and the theory of elevation, which I shall have to examine, will give rise to various important questions, both of natural philosophy, and of natural history.

35. There remains another question, which is of the greater importance in geology, as all others are connected with it, on account of the time which the supposed operations would require. What is the antiquity of our continents? In most of the theories of the earth, an incalculable antiquity has been necessarily assigned to our continents, to account for those supposed operations; and this is more especially requisite in the hypothesis that running waters have excavated the vallies among mountains, and the dales between the hills; an opinion which Dr. Hutton and Mr. Playfair enforce by all the means in their power. I shall therefore resume the subject of the antiquity of our continents, on which I had already dwelt at large in my first geological work; and I shall again prove, by the very operations of running waters, *that our continents are not of great antiquity.*

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36. The above stated questions, which have arisen in geology, are sufficient to give a general idea of the objects of this science, and of its extent; and it may likewise be seen, that the decision of these questions must, in a great measure, depend upon facts; which condition implies a precise determination of geological phenomena; to which attention can in no way be better excited, than by those questions themselves. Geological travels, no doubt, contain the descriptions of these phenomena, but all intermingled with each other; since the observations made in passing from place to place present objects of different kinds, and cannot be sufficiently impressed upon the mind, unless distinct heads are assigned, under which those of every separate class may respectively arrange themselves; and in order to effect this, the precise questions must be stated, which it is the purpose of those observations to resolve.

37. Most of the geological phenomena have at all times been open to general inspection; how then has it happened that men were so long without directing towards them a curious and attentive eye? because they had not yet conceived that the objects which they beheld must have before been very different. The first inhabitants of the earth, observing that in certain parts its surface was intersected with vallies and dales, while in others it was levelled into plains, contented themselves with distinguishing these different sorts of grounds by respective names, and studied them for the sole purpose of utility, without

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adverting to any state in which they might antecedently have existed.

38. The first object which excited astonishment in men, and by which it had already been excited in ancient times, was that of the marine shells found scattered in some countries, even at a very considerable distance from the sea. But, as traditions concerning a deluge had reached them, they connected, in a confused manner, the two ideas. Hence Ovid introduces Pythagoras as saying,

—— " Vidi factas ex æquore terras,
" Et procul a pelago conchæ jacuêre marinæ."

No researches, however, as to the nature or the causes of this change, were the result of that remark, because observation was not sufficiently advanced to suggest the necessity of pursuing that study. Time alone could, by accidental circumstances, bring natural history and natural philosophy to a point, at which they were capable of becoming sciences susceptible of gradual improvement by application; and much time elapsed before any rules were established to direct this application. Now, during the whole of that interval, imagination created systems upon systems, while, however, the collection of facts was daily increasing. Bacon at length appeared, whose profound genius enabled him to unravel that confused mass of casual observations, which was ill calculated to lead to inquiries, because the whole was imperfect, and devoid of any real connection. It

was he who pointed out the road which must be pursued, in order to trace back phenomena to their causes, and who, among other paths which he opened as leading to determinate objects, sketched a first plan of observations, which might give birth to geology. But men were too precipitate: before the observation of the actual state of the earth could lead with certainty to the knowledge of its past states, and manifest their causes, it was necessary that the study of the earth should have produced many new discoveries. Instead of which, some naturalists, elated by the beginning of this happy change, fancied that they heard on every side the voice of nature. Thus geological monuments, which had been mute before, were converted into oracles; and it was imagined that every thing was to be learnt from the first which seemed to speak. Hence the many systems that arose; each observer creating his own conformably to the object by which he had been struck. However geological views were thus formed, amidst objects, which before had been thought to possess no interest, except for the architect, who sought the stony strata best adapted to the purposes of building; the agriculturist, who looked for beds composed of fertilizing substances; the miner, who endeavoured to trace metallic veins; the landscape-painter, or the poet, who adorned their representations with rocks, cascades, and torrents, to form a contrast with more peaceful scenes.

39. Mr. Playfair, therefore, had reason to say that theories of the earth, however erroneous and discordant

discordant they formerly were, may nevertheless be considered as first steps in geology, on account of the phenomena collected by their authors, and of the views which they have opened; but the systems themselves ought not to have obtained confidence, before attention had been paid to the importance of the object, arising from the moral danger of the errors into which men might fall by too much precipitation. It is thus, however, that geology, gradually accumulating materials in all its branches, of which many had not at first been considered as necessary, is become a solid science. But these materials are as yet dispersed in various kinds of fabrics, where their bonds of union being arbitrary, they give to the science that vague appearance, which deters from the pursuit those who seek solidity in knowledge, while to others it presents a mere field of conjectures, in which they make no scruple of placing their own.

40. This state of things is solely to be attributed to inattention; for the field of geology is at this day much better known than it is generally supposed to be: there are true roads very accurately traced in it, which enable us to fix and characterise unalterable parts, sufficient for its main purpose, and point out openings to other paths, leading to objects which yet remain to be discovered.

41. Mr. Playfair has likewise justly observed, that one of the causes which have produced much confusion in geology, is evidently that too much has been attempted. But, in reducing its limits to the stand-

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ard of the theory which he has adopted, he has increased, instead of dissipating, this confusion; because he has excluded the lights resulting from certain well-determined origins. The only means of putting an end to the conflict of systems was to return to the phenomena themselves, in order to study them with redoubled attention; seeking first those, which might lead to immediate consequences with respect to their causes. It was thus only that the complication, so perplexing in these phenomena, could be unravelled; for by separately considering those proceeding from known causes, the parts, of which the causes were yet undiscovered, would appear divested of all accessory combinations.

42. My brother and I entered together upon our geological career, while I yet lived at Geneva; and after a certain period we came to a first conclusion, which, from that time, was our guide, and which I still consider as the true introduction into the field of geology. We saw that an essential distinction was to be made among the various phenomena which the surface of the earth exhibits, with respect to their causes; determining of each of them whether the causes which have produced it are *still in action*, or have, at some epoch, *ceased to act*. If this discrimination be possible, it evidently becomes a first guide in the research of causes, which will prevent many errors. Now, when we had fully convinced ourselves that this distinction was pointed out by the phenomena themselves, we clearly saw, on studying the theories of the earth then known, that the prin-

cipal source of the errors, which had been detected in them by subsequent observation, was the confounding of the periods in which certain effects had been produced. For, by attributing to causes which were seen in action, such effects as they were incapable of ever producing, an impenetrable veil was thrown over past causes; since these can be discovered only by their real effects, more surely to be ascertained when separated from those produced by causes, which are still operating, and producing such effects, as may be discerned to belong to them.

43. Continuing our observations with this object in view, we came at length to a conviction that the production of the mass of our continents, in regard both to their composition and general form, as well as their existence above the level of the sea, should be ascribed to causes no longer in action on our globe; and that the whole of the effects of actual causes had been limited to the modification of this original state. Insomuch that the first study requisite to the investigation of the past history of the earth, was that of the action of actual causes; that thus, by their being every where determined, our continents might be traced back to their original state.

44. After having quitted Geneva, I continued my observations in various countries, and more particularly on the coasts; my brother likewise pursued his; and we communicated our remarks to each other. I was more and more convinced, not only of the possibility

possibility of determining, by the action of actual causes, what had been the state of our continents at their birth, but even of the possibility of likewise determining the time elapsed since that period. To these objects I directed my researches, which enabled me more clearly to perceive the errors of all the geological systems formed without such investigation.

45. With these preliminary determinations, I wrote my first geological work, *Lettres Physiques et Morales sur l'Histoire de la Terre et de l'Homme*, published at the Hague in 1780. In this work, I determined the original state of our continents, and proved that they were of very small antiquity; a circumstance which alone overthrew a variety of systems, as I made it abundantly appear. But, when evidence can be obtained only by long and laborious observations, time is also requisite to render it manifest. Many eminent geologists, who, as well as myself, had extended their observations over a wide field, have acquiesced in those results, and have even supported them by new facts; whilst others, prepossessed in favour of certain hypotheses, and confining their observations to particular spots, have continued to frame systems suggested by imagination alone. Of this I shall adduce only a single example, but that a recent one, which will evince the importance of the distinction to be made between the causes, which have acted upon our continents since they have begun to exist, and those, to which are owing the principal characters possessed by them at their birth.

46. Dr. SCHMIEDER, of Halle, published in 1802 a work, entitled, *An Exposition of Geology upon Chemical Principles.* He supposes, with Dr. Hutton, that our continents have been raised up from the bottom of the sea; he imagines them to have possessed at their birth the great chains of mountains only, formed by the very act of their elevation; and he attributes to the causes, which from that time have been operating upon them, the mountains, which he calls secondary, where the remains of terrestrial animals and vegetables are found; an assumption, which gives to our continents an incalculable antiquity. It was easy to shew in how many respects this system was contradicted by phenomena observed on the surface of our globe, and this I did in a small work published at Brunswick in 1803, under the title of *Abrégé de Principes et de Faits concernans la Cosmologie et la Géologie;* in which I made it more particularly to appear that those mountains, which he supposed to have been formed upon our continents, subsequently to their birth, by the materials which running waters had detached from the high mountains, exhibited all the marks of submarine formation; that their strata had undergone the same catastrophes as those of the great mountains; that the nature of the latter is such, that the sediments of the waters which proceed from them cannot, on the same spots, produce strata, the species of which frequently and abruptly change; and lastly, that the period of the birth of our continents is not sufficiently remote for the immense time, which such operations would have required. Now, while Dr.

Dr. Hutton carries into the sea the materials of old continents, to form the strata of new ones, we see Dr. Schmieder considering the same operation as having been performed upon our continents themselves: this shews the variety of systems which may be formed on the same phenomena, when they are superficially observed. Dr. Schmieder answered me; I replied; but for the purpose only of announcing to him the details of observations, which would be contained in the collection of my travels; for I was already aware, (and it will form here one of the objects of my remarks) that the removal of prejudice can be effected only by the *details* of observations.

47. This example will suffice to explain what I stated above, that the first point necessary to be fixed in geology is the state of our continents at their birth; it shall form therefore one of my principal objects in the sequel of this work. It was chiefly by means of such an investigation, that hopes could be entertained of ascending to the causes, which have operated in time past for the production of the mass of our continents, as we now observe it; but this research was very difficult; and it called for certain assistance, which, at the time when I published my first work, had not yet been obtained; of this I was well aware, and I declared it. And first it was necessary to survey, with greater attention and exactness, the stupendous masses, which form the large chains of mountains on our continents, such as the Alps, the Pyrenees, and others of the same class.

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I had not as yet been enabled to form any opinion respecting the structure of this class of mountains, although they had frequently been the object of my observations; only, indeed, for the purpose of studying the changes which they had undergone since the birth of our continents, as their nature was to me involved in mystery; and this I acknowledged. In order, however, to avoid deciding that they were *primitive*, as some mineralogists termed them, or, in other words, that they had belonged to the earth from its origin, I called them *primordial*, as having been evidently anterior in existence to those, in which organised bodies are found.

48. The publication of M. DE SAUSSURE's second volume of his *Voyages dans les Alpes*, which appeared in 1786, formed the epoch of an important step made in geology. It was then that a new scene opened upon me, as if a veil, through which I had hitherto surveyed these monuments of our globe, had been suddenly withdrawn. For, from that time, I no longer considered the eminences called mountains, of any size, or of whatever substances composed, as masses, which had originally existed, or of which the materials had been produced under that form; but merely as *strata* of different genera and species, *successively* formed at the bottom of a *liquid;* an operation, which could be assigned only to chemical precipitations. One class indeed of these strata, commencing with the granite, having been produced antecedently to the existence of organised bodies upon our globe, might retain, not as mountains, but

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as strata only, the name of primordial, which I had given to the mountains themselves; and the term secondary might still be applied, not to mountains, but to those strata which had been produced subsequently to the existence of organised bodies; but with respect to the peculiar forms of mountains or hills, I look upon them as the effects of the catastrophes, which their strata had undergone, after the time of their original production.

49. I found the justness of M. de Saussure's remarks, with respect to the very mountains which I had already surveyed, and even earlier than he had done, but not under such favourable circumstances, and with the assistance since furnished by the progress of observation. These remarks, the truth of which I have had every where an opportunity of ascertaining, have permanently established a third point in geology. I say a *third* point, because the first which I have mentioned is the birth of our continents, produced by some revolution of the globe, and the second, the small distance of that epoch. But this third step leads us farther into past events; and this is to be attributed solely to M. de Saussure. It has, indeed, been contested by some geologists, and chiefly by those who consider mountains as being large crystals, an idea more particularly maintained by M. de la Metherie. But this opinion, as well as the notion of primordial mountains, could proceed from the want only of such a study of mountains as M. de Saussure has pursued, and can maintain its ground with those alone who have not paid sufficient

sufficient attention to his remarks. With regard to me, I was struck by them, as if a ray of light had broken forth from the mountains and the hills themselves: in no instance have I found any exception to them; and I am persuaded that the great geological fact which they have established will soon be universally admitted.

50. Dr. Hutton, who was well aware of this characteristic fact, and had bestowed merited commendation on M. de Saussure, has, however, controverted a part of this great discovery, by excluding from the number of stratified substances, those mineral productions which are rightly considered as having preceded all the others, namely, granite, porphyry, and other substances, found intermixed with them, and it is this opinion which impresses him with a belief, that it is impossible to ascend by geological monuments to a *primitive state of the globe.* This is a great question; and Mr. Playfair having supported the hypothesis of Dr. Hutton with respect to it, the acknowledged talents of these geologists, together with the important questions with which it is connected, will induce me to examine it deeply. This hypothesis is very different from that of M. de la Metherie, who admits at least a *first* discernible operation upon our globe, which he defines to be a crystallization in mountains. But it will appear from all the details into which an examination of Dr. Hutton's hypothesis will lead me, that both these theories are equally excluded by obvious phenomena; so that M. de Saussure's discoveries, with respect to the

the formation of our mineral strata, beginning with the granite, by chemical precipitations from a liquid which originally covered the globe, together with his explanation of the subsequent production of mountains and hills, remain in their full force. But anticipating the subject, and referring to the proofs which will be hereafter adduced, I shall set out from this point, and continue to sketch the objects which are embraced by geology.

51. Here then we have a first operation of physical causes on our globe, to which we are led back by the investigation of geological monuments; namely, the production of the strata of granite, by chemical precipitations, from a primordial liquid. Such is the first discernible effect of these causes; beyond it, we cannot perceive any thing: all the other effects of which monuments remain are subsequent to this, and may be expressed in the following general terms: They consist, in the first place, of other precipitations, continued for a long time, of substances, differing successively in their genera and species, and spread out in strata, nearly in an horizontal and continuous position, at the bottom of the liquid. But these strata, in the intervals of the formation of their different classes, and until the end of these operations, underwent great catastrophes, in the course of which they were fractured and dislocated. These revolutions at the bottom of the liquid, produced in the mass of the strata those relative differences of level, which constitute mountains and hills, rising above the vallies and the plains. Lastly, this theatre of

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operations and revolutions, having been left dry, became our *continents*.

52. What a different prospect was thus opened in geology! What hopes did it not excite of penetrating, by means of such distinct monuments, into the past history of the earth, as far, at least, as these monuments, with the assistance of physical causes, could direct us! Much indeed still remained to be done before these hopes could be realized, but every thing was preparing for new advances. Geological monuments, more and more the objects of study, were still undoubtedly to be our guides; but these, with regard to the time which preceded the birth of our continents, could exhibit to us the effects of causes only, which, though they have not ceased to exist on our globe, are yet no longer in action, because circumstances are changed. I had made great advances in the determination of these past effects, by carefully tracing those of the causes by which they are modified; this had determined the state of the different parts of our continents at their birth; and that state served to characterise the causes which had produced it. But, as these causes no longer act in the same manner, it became necessary to study them in the effects which they still produce, in order to discover their nature, and to judge what their action would be under given circumstances. This was the province of natural philosophy, and more particularly of chemistry, which is one of its most important branches; and fortunately, at the time that M. de Saussure opened so wide and important a field,

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experimental philosophy was rapidly advancing to discoveries, from which considerable assistance might be derived.

53. To proceed farther in this sketch of the objects which geology embraces, must involve me in a repetition of what I have detailed in a second work upon this subject, published at Paris in 1798, under the title of *Lettres sur l'Histoire Physique de la Terre, addressées à M. le Professeur Blumenbach*; I shall therefore here present a summary view of what this work has added to the former, *Lettres Physiques et Morales sur l'Histoire de la Terre et de l'Homme.*

54. I have already adverted to the error which prevailed respecting mountains, and impeded the way to the discovery of the causes that had acted upon the earth before the birth of our continents. I had called them primordial, and not primitive; which prevented error in one point, but did not remove it in its essential part. Nothing on the globe can, according to my former remark, be considered as having been originally produced under the form of mountains: for the substances, which compose the mass of these eminences, had been first disposed in horizontal and continuous strata; and the form of mountains, in which they are found broken and dislocated, has been the effect of subsequent catastrophes. Such is the system which I have adopted, guided by M. de Saussure, after having every where found the confirmation of it. My second work, therefore, contains an exposition of this system; and the

the principal new results to which it has led me are the following.

55. 1. When we trace back with attention the effects successively produced by the different causes, which must have acted upon our continents from their birth, and are still continuing to act, we are enabled to determine what was the state of these continents at that moment, and we become assured that their existence is due to a sudden retreat of the sea. 2. This original state, from the aspect of the vast ruins which characterise it, leads to the necessity of admitting, that, while the mineral strata which throughout constitute the mass of our continents, were accumulating at the bottom of the sea, vast caverns were either forming under them, or were already in existence, into which large portions of their masses sunk down, while other parts, inclined in different directions, remained more elevated; such catastrophes must frequently have been repeated. To these is to be ascribed the existence of our mountains and hills, including their vallies; the plains being the parts which had most considerably and extensively subsided; and at the same time large portions of the liquid penetrated into these caverns through the interstices of the fractured strata. 3. In consequence of this diminution in the quantity of the liquid externally, peninsulas and islands were formed in the basin of the ancient sea, which were supplied with vegetables and animals from other continents then existing. But during the continuance of the catastrophes at the bottom of the sea,

many of the peninsulas were cut into islands, and many of the islands sunk under the level of the waters, where they were covered with different species of mineral strata, which, with the preceding masses, underwent new catastrophes. Hence the numerous remains of terrestrial vegetables and animals, which are found, intermixed with marine bodies, in so many parts of our continents, in strata, or under strata, which, in common with all mountains, hills, and plains, bear the marks of convulsions frequently repeated. 4. A great number of these islands were still subsisting at the birth of our continents; so that, by the retreat of the sea, they became the summits of our mountains, and chiefly supplied our continents with terrestrial vegetables and animals. 5. The retreat of the sea, by which our continents were formed of that part of the globe which had originally been its bed, was effected by the sinking of those continents, whence were derived the vegetables and animals, with which the peninsulas and islands had been stocked. It was then that the sea, flowing over those sunk continents, left our own dry. 6. Since the period of that revolution, the level of the sea has not been changed. 7. Lastly, not many ages have elapsed, since that great terrestrial event took place.

56. Such are the immediate consequences of an attentive study of geological monuments; nothing in them is left to conjecture; every thing rests on phenomena every where observable, which will become the object of this work, and will form its only foundation,

tion. But nothing which has yet been said concerns those *past causes*, which were in action before the birth of our continents: these also I have developed in my second work; and I shall sketch the path which I pursued in this investigation, tracing the series of events under another point of view.

57. The strata of granite, evidently produced by chemical precipitation from a liquid, form, as I have said, the most ancient monument of the action of physical causes on our globe; we cannot, I mean, by the aid of any monuments, ascend to any more remote effect. To the first precipitations of this class, others succeeded, of different kinds; and while the strata were accumulating, they underwent the great catastrophes already mentioned, that gave birth to islands, some of which afterwards sunk. I have not as yet mentioned a circumstance, which does not enter into my immediate plan, but of which I have treated in my second geological work, namely, the birth of prior continents in the course of those revolutions, whence were derived the vegetables and animals found buried in our own. I have here in view only a series of events, commencing with the production of granite, succeeded by that of a great number of other species of mineral strata; a series intermixed with great catastrophes, the last of which consisted in the sinking of the first continents, and the birth of our own.

58. Then commenced another part of the same series of effects, all connected one with the other.

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Our continents, being once left dry, began to undergo the action of atmospherical causes, the effects of which were much increased by the ruinous state of their external parts. These effects were in general—the crumbling down of abrupt surfaces, of which the materials accumulated under them in the form of slopes;—the softening of rapid declivities by the rain waters, which, in flowing down their sides, carried along with them the loose and incoherent materials, till they were bound together by the roots of plants, as vegetation spread over them;—the filling up of cavities, and the raising and levelling of the bottoms of vallies, by the materials which these waters propelled in their rapid course, and deposited in the parts where their current slackened;—the levelling and straitening of the channels of these streams in the vallies and plains;—lastly, the action of the sea upon the coasts, where it attacked the steep parts that opposed resistance to its waves, the force of which had a tendency to wear away capes, fill up creeks and bays, and reduce cliffs to gentle declivities. Such are, I repeat, in a general point of view, the effects produced by the action of atmospherical causes upon our continents, operations which commenced from their birth. I have shewn that all these effects tend, by their very nature, to special ends, the completion of which, in respect to each, I have pointed out in a variety of places. There are many spots, however, where these effects are not yet terminated, although it evidently appears in what manner they will end.

59. If this brief sketch of the physical operations which are observable on our globe be in reality traced out by their monuments, as I propose to prove from the monuments themselves, it will hence inevitably follow, that all the perceptible effects of physical causes upon the earth must have commenced at a certain period, indicating the accession of some *new cause*. For, since the most remote effect of which any monument remains, namely, the production of the strata of granite, has been followed by an uninterrupted series of other effects, which, though tending towards an end, is not yet terminated, a certain period in the history of the earth is evidently pointed out to us, at which a cause, before wanting, began to exist, of which the production of granite was the first operation; and the investigation of this cause thus becomes an important object to the physical geologist, since it clearly belongs to the department of natural philosophy.

60. It was chiefly after having attained this point in its progress, that geology stood in need of the aid of chemistry; but it did not at first receive that aid; and some geologists, when considering this object, of the first operations which took place on our globe, prematurely decided on the possibility or impossibility of deriving any assistance from that science, in the elucidation of effects so remote from our times. It is thus, for instance, that Dr. Schmieder, whom I have already mentioned, has thought chemistry able to furnish even specific explanations of these operations; and it was on the contrary, with as little

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reason, maintained by Dr. Hutton, that no assistance could be expected from this science. It is doubtless very evident that no specific affinity can be assigned, as has been attempted by Dr. Schmieder, to a period, when all the ingredients, now variously dispersed in our mineral strata, in the atmosphere, and in the sea, were combined together in the mass, which, in process of time, came to be thus decomposed; for most of the mineral substances, from which we derive our chemical conclusions, are compounds, which indeed we are able to decompose to a certain degree, but which hitherto we want means to recompose. This evinces the little depth of our analyses, and proves that we are not acquainted with all their products. But if the details of specific affinities, as yet furnished by chemistry, can afford us no assistance, while we attempt to particularise the first operations which took place on our globe, this is by no means the case with the general laws which have been determined in that science; for they lead us to generic operations, whence much light is thrown upon these beginnings. This is an important subject, of which I have introduced only the principal heads in my Letters to Professor Blumenbach, referring, as I must do here, to the work which I have since published at Paris, under the title of *Introduction à la Physique Terrestre par les Fluides expansibles*, the purport of which only I shall mention here.

61. I shall set out with the proposition, that all our mineral substances, beginning with granite, are chemical precipitations from a liquid, which originally covered

covered the whole surface of the globe; a principle which I have adopted in common with the most eminent geologists; and I shall have an opportunity of farther supporting it, when I shall discuss the objections of Dr. Hutton. Now when a liquid contains ingredients capable of union by their affinities, and of subsequent separation from it by precipitation, such union and such separation must immediately take place; for, as soon as causes exist, effects are produced. The following, then, is the first question which presents itself,—Why, at the period to which we have ascended by means of geological monuments, had not the ingredients of granite already been precipitated? And here we are reminded of a general law, long established in chemistry, viz. that no chemical combination in a continuous mass can take place without liquidity, to permit the ingredients to approximate each other, and coalesce by the surfaces which have a tendency to unite. The answer, therefore, to this first question undoubtedly is, that, at the time when granite had not been yet precipitated, the mass, containing its ingredients, was not liquid. Thus the change which then took place in this mass consisted in its acquisition of *liquidity*.

62. A second question then to be resolved is the following:—Why was not that mass already liquid? Another general law furnishes us with the solution of this question. It is now established as a fact in chemistry, and in natural philosophy, that the liquidity of every substance is exclusively produced by a certain union of the calorific fluid with its molecules;

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an union by which this fluid, known to former natural philosophers under the name of fire, and among some of the moderns by that of caloric, loses its faculty of producing heat, while the other substance loses the distinctive properties of solids, and assumes those of liquids. The following, then, is the answer to this second question:—At the time when the mass, from which granite first separated itself, was not liquid, it did not possess the *fire*, which produces liquidity.

63. I shall dwell for a moment upon this point, on account of the misconception of it by M. de la Métherie, who had not examined in my works the propositions which lead to it. He has given (p. 381, of the third volume of his *Theory of the Earth)* of the whole of my system, a rapid sketch, which it was easy to render susceptible of his objections; but I shall content myself with copying that part, which relates to the present subject. The following are the terms in which he professes to set forth my opinion: " The earth was *congealed* . . . . . the sun becomes " luminous; its light, darting upon the surface of " the earth, heats and thaws it. The waters, pro- " duced by the melting of the *ice*, penetrate into the " interior parts of the earth, which he conceives to " have been a mass of humid dust congealed. The " heat of the solar rays increasing on the surface of " the globe, the dissolution of the ice becomes more " considerable . . . . the waters dissolve the earths, " and other congealed substances . . . . lastly, when " this dissolution extends to the depth of several

" leagues

" leagues below the surface, the substances, dissolved " by the waters, crystallize, are deposited by preci- " pitation, and the external crust of the globe be- " comes solid. Such is the origin of the primitive " lands . . . . ." I consider M. de la Métherie as having shewn me great indulgence in not pronouncing such a system to be altogether absurd; but his attention has not been equal to his indulgence, for the system of which he gives this exposition bears no resemblance to mine. But I shall not stop to prove a dissimilitude, which may be seen in his own *Journal de Physique*, and especially in my *Letters to Professor Blumenbach*. One point only I shall explain, which will effectually remove this idea of ice and congealed substances, so materially differing from that which forms a part of my system.

64. There exists on our globe a very important substance, which should have a name appropriate to its intrinsic nature. I had proposed terming it *humor*; but that neologism was not adopted, because its utility had not attracted general attention. It is only since the period of the first operations which took place on our globe, that this substance has appeared as separated from the mass; and it is now to be observed under three distinct forms. It is *water*, when united to the fire of liquefaction; it is *ice*, when, after having been liquid, it crystallizes in consequence of being deprived of that fire; it is *aqueous vapour*, when its molecules have been detached from the mass, whether water or ice, by the impulse of the particles of fire, which then remain united to them.

65. Such

65. Such is the substance which I have called *humor*, and to which a distinctive appellation should certainly be assigned. It is not water, for in water it is liquefied by a certain union with fire; it is not ice, for in ice it becomes crystallized, after having passed through the liquid state; lastly, it is not aqueous vapour, for in that state it has acquired expansibility, by another species of union with fire. Many chemists still conceive this substance to be a compound; and I considered it so myself, at the time when some well-known experiments gave rise to that opinion. But philosophers were too hasty in fixing by nomenclatures a point so important in terrestrial physics; for numerous facts, and particularly atmospherical phenomena, have restored to this substance its elementary nature. We no longer perceive it in its original state; it has formed, by its combinations, a great part of the mass of discernible substances upon our globe; it continues, by its affinities, to distribute itself, in most of the products which we extract from them by our analyses; and the most simple mode of its existence is under the three forms which I have designated above. But in its primitive state, when nothing of what we now observe upon the earth was produced, nor as yet disposed to be produced, this substance was neither water, nor ice, nor aqueous vapour; its elementary particles were intermixed with all those, to which they are at present united by affinity, in the different bodies with which we are acquainted; and those particles, then acquiring liquidity by their union with fire, and thus immediately entering into all the associations, to which its numerous

numerous affinities gave birth among elementary substances, produced the primordial liquid. From this liquid, our mineral strata, and the atmosphere, were successively separated, by other elements, which arose from the interior parts of the globe, leaving for residuum the waters of the present sea. This is what I have developed in my Letters to Professor Blumenbach.

66. But a third question thence arises: Whence proceeded the fire, which produced liquidity in that first congeries of elements, forming, to a considerable thickness, the external mass of the earth, from which has proceeded every thing we see, and beyond which we can penetrate only by means of inferences drawn from perceptible phenomena? And here we are still assisted by natural philosophy. All the experiments and observations relative to fire have led us to conclude, that this fluid, subtle as it is, is not a simple substance; that it is composed of light, and of another substance belonging to the atmosphere, and to terrestrial bodies; and that the solar rays, in uniting themselves with this latter substance, produce heat upon our globe, but only by the formation of fire. This substance, therefore, might be intermixed with all the other elements which formed the primitive mass of the earth, without producing heat, or, consequently, liquidity; for it was not fire itself, it was only one of its ingredients, which, without the addition of light, could not constitute fire. The solution then of this third question is, that, before the epoch when the mass of elements,

whence

whence has been produced all that we observe upon our globe, began its operations, *light*, that essential element, was wanting to it, for the production of fire, and, by its means, of liquidity.

66. *(a.)* I am thus arrived at the fixed period, at which commenced the production of the strata of granite, that most ancient monument of the action of physical causes on our globe, and the first of the effects in the series which I have traced, extending to our times, but not yet terminated. In my inquiry into the causes which had prevented their beginning, I have proceeded, and could proceed only by the help of general laws, resulting from every branch of natural philosophy, and particularly from chemistry. Here therefore I stop, as this guide cannot lead me any farther. It would now be requisite to determine whence proceeded light, that chemical ingredient, which has entered into the composition of many mineral substances, since it is disengaged in their decomposition; and which, in the first place, was to produce fire, and next, by fire, to produce liquidity; but of that light I can perceive no source. It would be removing the difficulty only a little farther, to derive it from the luminous celestial bodies; for whence did they receive the light which emanates from them? Here then terminates the field of our researches; but this limit does not affect the series of inferences from facts, by which we have been led so far. It is certain that, when none of those operations of physical causes, of which we observe the monuments upon our globe, had yet commenced, light,

light, as a chemical ingredient, was wanting to its mass. For, if light had always existed there, it would have produced fire, and fire, liquidity; thence would have resulted all the various precipitations, the catastrophes of strata, the birth of our continents, and the action of atmospherical causes upon them; and these last effects would every where have been terminated, so that we should have been almost unable to trace back any of the links of their series.

67. It is thus that by analytically tracing back the chain of phenomena, and of their general causes, we arrive at the formation of the strata of granite, as the most ancient perceptible monument of these causes. In our redescent from this point, we find a succession of other strata, different in their genera and species, formed in the same liquid, and impressed with characters, which, independently on their superposition, indicate different periods of formation; as more particularly appears from the remains of organised bodies, terrestrial as well as marine, which they began, after a certain time, to enclose. All these strata, moreover, have successively undergone considerable catastrophes, to which they continued subject until the birth of our continents. But, from this period, all those great effects have ceased on our globe. The residuum of the primordial liquid, which is the sea, no longer produces mineral strata; it has lost all the substances which produced those of which the mass of our continents is composed; and new ingredients, capable of effecting such precipitations, no longer arise from the interior parts of the earth.

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The mass of the strata, now abandoned by the sea, is no longer subject to those catastrophes, by which their preceding revolutions had been occasioned; the interior of the globe having already assumed a fixed state. Terrestrial causes still remain the same, but circumstances are changed: their general characters, indeed, are determinable by their actual effects; but the latter mostly consist in those vicissitudes of composition and decomposition, which we observe on the surface of the earth.

68. I have hitherto omitted speaking of *volcanoes*. They are a distinct process, connected, no doubt, with the primitive composition of the globe, their monuments being even intermixed with those of precipitations, and of catastrophes of the mineral strata; but they have not contributed to these effects in any way that has yet been discovered. Dr. Hutton is of a different opinion; and I shall examine his reasons, as I have examined those of Dr. Schmieder, who thinks, with Dr. Hutton, that our continents have been raised up by a subterraneous heat, though he differs widely from him in every other part of his theory.

69. The monuments of effects, posterior to the formation of the strata of granite, are our guides in the determination of other circumstances relating to the nature of the mass of the earth, at the period when all these operations began; for it must have contained within itself the causes, which produced the successive differences of precipitations from the

same

same liquid, as well as the cause of the catastrophes, which the mineral strata have undergone since their production. It may easily be conceived that inferences can be drawn with a probability approaching to certainty, only by taking in the whole assemblage of well-determined phenomena; and such is the method which I have followed in my Letters to Professor Blumenbach. I have formed no conclusions arbitrarily, they proceed from the facts themselves; but these are so numerous, and so varied, that it would be necessary, were I to detail them here, to go over all the branches of geology, which it will be more proper to refer to the sequel of this work; for the present, therefore, I shall indicate only one of the principal of these branches.

70. The cause of the catastrophes of mineral strata, and that of the birth of our continents, which refers to them, cannot but be intimately connected with all other geological phenomena; and it is necessary, in the first place, to determine, with respect to them, whether, in the changes of relative level of such masses of those strata which have formed even the great chains of mountains, (changes which are the consequenees of those catastrophes) the parts most elevated have been raised, or the lowest have subsided. The former of these systems, as we have seen, is maintained by Dr. Hutton and Mr. Playfair: the latter is mine; and this difference of opinion will give rise to an attentive examination of them.

71. Both these theories necessarily imply the existence of *caverns* in the interior of the globe; nothing but the admission of caverns can possibly account for the operations, which form a part of each system. Now as, of all the hypotheses which have been framed for the explanation of terrestrial phenomena, these two can alone maintain so far their ground, the existence of caverns in the interior parts of the globe forms a fixed point in geology; and the origin of these caverns being necessarily connected with the primary constitution of the earth, they become a leading step towards its determination. But a great difference occurs, accordingly as either of these systems is embraced. In the theory of subsidence, which, in consequence of some striking symptoms, I had already adopted in my first work, but which has been considerably developed by subsequent observations, the formation of these cavities is a consequence of that state of things, which produced different successive precipitations from the same liquid, as well as many other corresponding phenomena, which I have indicated. This theory makes it likewise appear why the catastrophes, which evidently took place at different periods, have been thus successive, viz. because the caverns themselves must have been formed in succession, from the very nature of the cause which produced them. Lastly, the same theory accounts for the stability of our continents, although they are only a mass of ruins, by implying that the great masses, as they sunk, descended to the bottom of the caverns; which therefore, at the close of these

 catastrophes,

catastrophes, were nearly filled up; such vacant spaces alone remaining in them, as occasion the present phenomena of volcanoes and earthquakes. Every thing is connected in this theory, and each of its parts may be submitted to the test of direct facts. But widely different is the case in the theory of elevation. And first, the hypothesis of a great internal heat, upon which it is grounded, is altogether inadequate to the explanation of the great phenomenon of mineral strata, successively differing in genera and species, and formed in the same places, from the same liquid. And as, according to this theory, the caverns must have been formed by the very act of elevation, they would necessarily still exist, and our continents, although composed of the ruins of strata, would in a manner be suspended over them. Now such being Dr. Hutton's system, a comparative examination of it with mine will give rise to various disquisitions relative to the primitive constitution of our globe.

72. I have now indicated the principal roads which have been opened in the field of geology, together with the different opinions still maintained in regard to them. This first sketch was necessary, in order that the reader might have a clear idea of the various parts of this field, when I come to treat of the questions connected with them. It may already have appeared, from this exposition, that a fundamental question in geology, nay, the first and most important in the actual state of the science, is the following: Can it with certainty be determined what was the state

state of our continents at their birth? That is to say, are the causes, which act and must have acted upon them from the beginning, together with the effects which those causes have produced, sufficiently known, to enable us to infer certainly from them what was the original state of each part of their surface? This then is one of the most essential objects of geological investigation, and it is likewise that to which the facts collected in my travels principally relate. In the rapid though correct sketch which I have here given of geology, I have been unable to introduce this great object, otherwise than in its relations to others, which I have also traced in a summary manner; but it will be developed in the following Dissertation, in which will be more distinctly seen the advantage of having in view a different theory, such as that of Dr. Hutton, supported by Mr. Playfair, on account of the talents and the knowledge of these geologists. For, as I have already remarked, it is the discussion of different opinions respecting the same objects, while the decision between them depends on facts, which fixes the attention on those objects; and as, with regard to these indispensable means of advancing in real science, the deductions from data, the above authors appear always to proceed with the most guarded caution, I shall also endeavour not to be deficient in this respect. Thus, I hope, considerable light will be thrown on the subject, and some fixed and unalterable points be established in geology.

DISSERTATION

On the Geological Phenomena which determine THE STATE OF OUR CONTINENTS AT THEIR BIRTH; *preceded by some general Considerations on the Causes which retard Decisions in the Study of Nature, and containing an Examination of Dr.* HUTTON'S THEORY OF THE EARTH, *with some Remarks on that of Mr.* KIRWAN.

73. THE principal object of this Dissertation is to be considered as a *central point*, to which all the phenomena scattered over the wide field of the travels of a geological observer ought uniformly to tend; as such, at least, I have in mine kept it constantly in view. It is for want of considering this subject, that the monuments of the action of physical causes, every where observed upon our globe, have given rise to so many different systems: for, until the importance of the distinction which I have pointed out between two classes of effects, one of those which had been already produced upon the earth's surface, *previously* to the birth of our continents, and the other, of such as took place upon these continents, *subsequently* to that period, be fully understood, and those of the latter class accurately determined, we remain in the same labyrinth in which the first geologists wandered.

74. There is but one way of ascertaining the original state of our continents; but it is one which cannot fail, provided the necessary time and attention be bestowed upon it: I mean the study of the physical causes, which are now acting, and must from their birth have acted, on them, together with the determination of what these causes have already effected. If this study be prosecuted with all the care which so vast a field of inquiry demands, each distinctive part of the earth's surface may be brought before the mind, in the state in which it existed at the origin of our continents, and the monuments of the action of *past causes* may hence be evidently discerned in them; causes more easily to be discovered, when thus characterised by their precise effects. Another great advantage results from such a mode of inquiry, viz. that by tracing back the actions of *actual causes*, we are enabled to ascertain, with sufficient precision, the time during which they have operated, or, in other words, the time which has elapsed since the birth of our continents, to set aside all systems founded in simple conjectures; which, requiring for their support a great antiquity in our continents, have thrown much obscurity over all terrestrial causes.

75. It is thus that the exact determination of the state of our continents at their birth becomes a central point in geology, because equally directed to *time past and present*. Now as this determination depends on the investigation of the actual state of our continents, and of the actions of causes which are constantly operating, it appears surprising that it should

should still remain a problem. This forms the subject of the remarks announced in the title of the present Dissertation; remarks, which will disclose one of the causes of that difference of opinion, still subsisting with regard to this point.

76. It is only by long experience in observation, that we are taught the necessity of repeating it, with respect to the same phenomena, when accompanied by different circumstances, for the purpose either of drawing inferences from them ourselves, or of learning properly to appreciate those of others. I was sensible of this, while frequently reflecting on the objects which presented themselves successively to my view in the course of my various travels, even of such as I had not undertaken for the express purpose of observation; for the more these objects were multiplied in my journals, or in my memory, the more I was convinced of the necessity of again examining those which I had already observed, not only by seeking for them in new places, but by returning to such as I had formerly visited, when other observers had deduced from them inferences which differed from my own. I have hence been induced to delay for so considerable a time the publication of the travels which I have frequently announced, having found it necessary to continue them, as long as I should have sufficient strength to bear the fatigue. Experience had taught me that it was impossible too often to inspect the same objects, with an eye which repeated observation had rendered more and more attentive, when the different aspects under which they

they might be viewed, had given rise to different systems; for no complicated objects can be fully known, unless that which is accessory to the character of their respective classes has been clearly discerned.

77. When the phenomena are of a nature to be found in different associations with other phenomena, if we wish to trace them to their causes, we must observe them in all these associations, and compare them under all these points of view. For phenomena, insulated and dispersed, like the scattered fragments of statues, easily enable the imagination to form, by the aid of hypotheses, groups differing as widely from each other as from the truth. It is only, therefore, after having repeatedly observed the same phenomena, under all their various aspects, and in all the combinations of which they are susceptible, that we can become acquainted with their specific characters; for, if well determined, they will always be found the same, in all their associations with other phenomena, of which, in their turn, they will serve to determine the proper characteristics; and this, at the same time, is the only means of ascending with certainty to their respective causes, which causes are themselves found to be mutually connected by corresponding links.

78. It is not possible, however, to have at once present to the mind these different representations of the same objects, though their distinctive characters can be ascertained only when they are viewed together; nor, indeed, does the mind enter upon such a process, when

when it draws conclusions; it generalizes them, under those characters, and forms groups, which become its immediate data. Now it is during this transition from the detail of phenomena, (which may be considered as the first data afforded by nature) to their generalization by the understanding, that arises the greater part of its errors; and these are the fruit of its impatience: it is eager to generalize, and that, not unfrequently without having collected a sufficient number of circumstances relative to the same phenomena, to enable it to ascertain their true and distinctive characters: their real connections thus remain unknown, and it is no longer the understanding but the imagination, that is at work, which at all times can combine and associate things, in themselves the most incongruous and discordant.

79. It is ever necessary to consult Bacon, as our true guide in the study of nature; for it was he, who, having collected together all that had been attempted in this career, discovered the real causes why so little progress had been made in it, and who pointed out the road to be pursued for avoiding error. With him, generalizations became a most important object. Perceiving the strong tendency of the mind to generalize, whether well or ill, in order to form a kind of science, he attempted to restrain it from employing its powers too soon upon abstractions, by showing how much was previously to be done. At the same time, he combated that scepticism, which refused to the human understanding a capacity of arriving at the truth, only because it had not yet followed the

roads

roads which might lead to it; roads, which he himself traced out with astonishing sagacity. It is chiefly, then, in regard to generalizations, that they, who devote themselves to the pursuit of natural science, stand in need of such a guide; for there, as I have said, originate the errors which produce uncertainty in that science; and, in pointing out their various sources, Bacon indicated one which is very common in the following passage of his *Impetus Philosophici; de Interpretatione Naturæ, Sent.* 8. "A philosopher," he says, "who is engaged only in the research of the causes of obvious and *compounded* phenomena . . . . . but does not apply himself to the discovery of *simple* phenomena, first to those which are such in the popular estimation, then to those which are reduced to a true *decomposition* and *simplicity*, by a kind of distillation, may, if in other respects he commits no error, add to what is already discovered something not to be despised, and even bordering on discovery; but will have no success in conquering the inveterate and general prejudices of the age; and cannot be called an *interpreter of nature* *.

80. It is this *decomposition* which must precede all generalizations; for these must refer to simplified phenomena,

* Qui in rerum obviarum et compositarum causis exquirendis, ... versabitur, nec se ad naturas simplices conferet, ad istas primo, quæ populari ratione tales sunt; deinde etiam ad eas quæ arte ad veriorem simplicitatem reductæ sunt et veluti sublimatæ, is fortasse si cætera non peccat, addet inventis quædam non spernenda, et inventis proxima. Sed nil contra majores rerum secularitates movebit, nec interpres dicendus erit. Vol. V. p. 186.

phenomena, if we are desirous of ascending, by their means, to *causes*. Yet, notwithstanding what Bacon had said upon this subject in many other parts of his works, it is still too common to generalize complicated phenomena, because they then lead us more immediately to practical consequences, which, no doubt, are very useful. But Bacon frequently and expressly attempted to counteract this propensity in those who devote themselves to the study of nature; because, in endeavouring to discover the practical uses to be derived from phenomena, they do not renounce the determination of causes, though they frequently profess to do so. Such a mode of proceeding is the more dangerous, as conjectures on general objects are supposed to be verified by successful, though only particular, applications.

81. In order to lay a sure foundation for some general theory, we must study with equal care all the phenomena, however remote, that are connected, by means of intermediate circumstances, with those, on which, as being principally in our view, we had bestowed a considerable degree of attention. For all these relations between objects, distant as they may be, originate in common causes; amongst which the most remote do nevertheless modify the effects of such as are more immediate in the phenomena, whence conclusions, relative to their distinctive causes, are drawn. Now if, previously to drawing these conclusions, no care has been taken to distinguish in these phenomena what it is which produces their relation to others, in order that such only of their

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circumstances may be viewed, as have originated in specific causes, erroneous determinations, with regard to these latter, will naturally be formed, the whole of an effect being attributed to them, which no single cause can produce. It then also happens that other observers, noticing the same phenomena under different associations, assign to them different causes; and this produces a general scepticism with respect to *causes* in the minds of those philosophers, who derive all their knowledge of natural science from books.

81. *(a.)* This object of generalizations, therefore, was, very justly, that on which Bacon dwelt the most, and more particularly in his immortal work, the *Novum Organum;* because it is the generalization of generalizations which furnishes the science of nature with solid general principles, continuing to rest on the phenomena themselves; and the assemblage of such general principles by degrees gives rise to the only metaphysics, which that great philosopher considered as real, or within the reach of the human mind. There is indeed much labour to be expected in pursuing the road which he prescribes; but he frequently adduces new motives of encouragement, by holding out the prospect of unexpected acquisitions of knowledge, which will undoubtedly be obtained in the course of such a mode of inquiry. Our way becomes less intricate, as we advance, a more extensive view opens before us, while our steps become more and more secure; and besides, this is the only road which leads to an outlet. "Let no man," (as he

he observes, Nov. Org. Aphor. 112.) "be alarmed "at the multitude of the objects presented to his "attention, for it is this, on the contrary, which "ought rather to awaken hope. For there are cer- "tain phenomena, both of nature and of art, which, "when, according to the evidence obtained, they "have been separated and abstracted from extra- "neous objects, may be collected as it were in a "handful, for the understanding to form its conclu- "sions from them. Of this road, therefore, the "issue is open, and even near; while the other" (that of speculations and comments on objects, such as they present themselves to the first view) "has no "issue, but becomes more intricate, the farther it "proceeds."

82. This is, therefore, the true road which we must follow, in order to promote the advancement of the science of nature; but it is not absolutely the difficulty of the path which retards the progress of this science: it still advances, though it spreads but slowly; and that from a cause which I shall notice here, as it will often become the subject of my remarks. The naturalist, who, in generalizing the phenomena which he has observed, shall have followed, in every respect, the admirable rules laid down by Bacon, will have present to his mind a miniature sketch of the phenomena which he has collected; and if he has been led to any conclusions relative to causes, he will perceive clearly that they are derived from the phenomena themselves, viewed in all their *details*. Subsequent observation, indeed, may lead to

to farther inferences, but his first decisions will re-main unchanged; because his conclusions have not gone beyond his data. However, he must not expect that his generalizations will impress others with the same conviction which he feels himself, although it is evident, that, provided they be just, his conclusions must inevitably follow; for generalizations do not bear within themselves any intrinsic characters of truth; since they are merely simple assertions of the observer relative to the nature of phenomena, and not these phenomena themselves. Now as it is very common to meet with different generalizations of the same phenomena, offered with equal confidence, that abstract form has lost its force, as well in the estimation of those, who, without prosecuting inquiries themselves in natural science, are yet desirous of knowing the different facts which have been ascertained, as of those, who have differently generalized the same phenomena, and who, half conscious, perhaps, that their own generalizations are not free from admixture of arbitrary determinations, imagine that the case of all other observers is probably the same as their own.

83. We may then easily understand what in fact is requisite to render the progress of natural science less subject to fluctuation; namely, that all philosophers, who offer generalizations as the foundation of any system, should form them, as it were, with the constant concurrence of their readers, by laying before their eyes all the details of the phenomena. But the fear of being tedious preventing many, who are well qualified for the task, from adopting such a course,

course, they are thus reduced to the level of those, who have formed their generalizations too precipitately; and hence all abstractions of phenomena are received with distrust. Consequently, real and general advances will then only be made in the science of nature, when the dread of prolixity shall be overcome, and when those who desire to cultivate that science shall be taught not only to wish for, but to expect, such a complete exposition of the several details of phenomena, as may enable them to enter by themselves upon the task of generalization.

84. It will indeed be perceived that I have been led to these remarks, the solidity of which cannot, I think, be questioned, in order to justify the details contained in the Travels, the publication of which will follow that of the present work. They have been accumulating for the last five and twenty years; and I have had many opportunities, during that time, of perceiving the little effect of generalizations, of which I shall have a striking example to adduce.

85. The object announced in the title of this Dissertation is the determination of the state of our continents at their origin; and I have shown that, until this point be positively ascertained, the science of geology must necessarily be vague and uncertain; for the causes, which have produced the mass of our continents, such as we observe it, will still be confounded with those which have altered their appearance since their birth. The only mode, as I have already said, of prosecuting successful inquiries on

this

this subject, is to investigate the causes *now operating*, and to trace the *changes* which they have already produced. For if this method be scrupulously observed, throughout all the classes of phenomena, if we thus come to ascertain the changes, which all the distinctive parts of our continents have undergone since they have existed, we may learn with certainty what their *original state* must have been. By the various effects, also, resulting from those several causes which we see in action, we shall be enabled to determine the period of time during which these causes have operated, and consequently *the real antiquity* of our continents; and these are the subjects of the present investigation.

86. That all geological phenomena are to be considered with respect to these determinations, is evident from the circumstance, that they are employed with this view in all the geological systems entitled to any attention. This is more particularly the case in Dr. Hutton's Theory of the Earth, which, as I have professed, I shall always have in sight in these inquiries. That geologist has, in fact, determined the state of our continents at their birth, by describing the several changes, which he supposes to have been produced by the causes acting on them since that period; and at the same time that, by his mode of considering the action of those causes, he is led to conclude that our continents are of immeasurable antiquity, he deduces from these operations the cause of the formation of our mineral strata at the bottom of the sea; and thus is obliged to have recourse to

the process, to which he ascribes their becoming dry land, under their present form; a succession of supposed effects embracing the whole field of geology. The outlines of this theory have been traced in § 24.

87. I have already said that Dr. Hutton did me the honour of sending to me his first paper on this subject, published among those of the Edinburgh Society; and that, considering myself as under the obligation of examining it with care, I addressed to him four *Letters*, which were inserted in the *Monthly Review* of 1790 and 1791. In these Letters I embraced all the principal propositions of his theory, and in particular those relating to my present object, viz. *the state of our continents at their birth.* This subject I shall at present resume, in the examination of Mr. Playfair's work, which gives a very clear exposition of the opinions of Dr. Hutton, as more fully developed, and more particularly defended, by that gentleman himself, in the second work published by him in 1795; and as I proceed in that examination, I shall occasionally recur to what I had opposed to him in my Letters, on his first statement of his theory.

88. In the course of these discussions, the truth of a remark, which I have already made respecting generalizations, will abundantly appear, viz. how little they are calculated to persuade, or even to engage attention. Although my first geological work contained many travels, it presented but few details, as I had chiefly confined myself to exhibiting phenomena

mena under general points of view. When I wrote my Letters to Dr. Hutton, I was not prepared for the publication of my new travels, both on account of other objects on which I was employed, and because I had farther travels in contemplation. In order, therefore, to combat his ideas respecting the effects of the causes which act on our continents, I had adduced the generalizations only of the phenomena that I had already observed; which, however, if they were correct, must have satisfactorily proved that Dr. Hutton had not paid sufficient attention to those phenomena. Now it is solely to this form of generalization that I can attribute the little notice which he, and also Mr. Playfair, who is acquainted with those Letters, have taken of them; so that neither of these gentlemen, while repeating the same opinions on this subject, contained in Dr. Hutton's first paper, has at all adverted to the facts which I opposed to them, although they themselves generalized the greater part of the facts on which they rested their opinions.

89. Having had, in my Letters to Dr. Hutton, more in view the examination of his theory than the exposition of my own, I concluded those Letters in the following manner:—"I think, Sir, I have now "completely executed what I had undertaken in "respect to your opinion on the revolutions that "have happened in our globe; but there remains "another point which I have not yet treated. "Thinking that you had explained very naturally, "and without any great destruction of the organised "beings,

"beings, the deep marks of revolutions impressed
"on our continents, you concluded thus: 'There-
'fore there is no occasion for having recourse to any
'*destructive accident* in nature, or to the *agency of*
'*any preternatural cause*, in explaining that which
'actually appears.' This point then remains to be
"examined, and I intend to do it as soon as some
"other avocations shall leave me sufficient leisure."
I fulfilled this engagement, two years afterwards, in my *Letters to Professor Blumenbach*, who undertook to translate them into German, in order to insert them in the *Journal de Phys. de Gotha;* but they were at the same time published in London, 1793, 1794, and 1795, in the British Critic. These are the same which I have already mentioned as having been published with additions at Paris, in 1798. As this last edition contains the exposition of the whole of my system, it is a work to which I am here under the necessity of referring; for I am now performing only what I then announced as my intention, viz. to support and confirm it in all its parts, by a further developement of facts, and their geological consequences.

90. Dr. Hutton was acquainted with my *Lettres sur l'Histoire de la Terre et de l'Homme*, in which the evidence adduced to prove the small antiquity of our continents was such, as forcibly to strike geologists, undoubtedly of the first eminence, since in their number I include Messrs. DE DOLOMIEU and DE SAUSSURE. I shall hereafter have occasion to quote the latter, on a subject upon which his authority is decisive;

decisive; but I shall state in this place the opinion of M. de Dolomieu, as contained in a note of his important Memoir *sur les pierres composées et sur les roches, (Journ. de Phys. de Paris, de 1792 Part I. p. 42.)* "I shall defend," he says, "another "truth, which appears to me incontestable, and "which the works of Mr. De Luc have rendered "evident to me; a truth, of which I see the proof in "every page of the *history of man*, and in all those "wherein the *phenomena of nature* are recorded "With Mr. De Luc, therefore, I shall say, that *the* "*actual state of our continents is not ancient*; with "him I think that *but a short period of time has* "*elapsed*, since they were given, or restored thus "modified, to the dominion of man."

91. I was already informed of the opinion entertained by M. de Dolomieu on this subject, having seen him at Paris in 1782, when I received Dr. Hutton's first paper. I was surprised that the latter should have bestowed so little attention on the proofs of various kinds which I had given in my work, of the small antiquity of our continents; but I ascribed it to the force of prejudice produced by his other hypotheses, which required that a period of time immensely long should have elapsed, since they had emerged from the sea. I therefore examined the hypotheses themselves, in the first of my Letters; and having there shown their dependence on the various operations taking place on our continents, it was the object of my three following Letters to compare those hypotheses with a general view of the phenomena

phenomena already described in my first work, which had been confirmed by many other observations made subsequently to its publication.

92. Since that time I have continued and greatly extended my observations, without finding reason to change any of the propositions contained in my Letters to Dr. Hutton, whether in relation to the phenomena exhibited by our continents, or to their geological consequences; and I believe that all those, who shall read them with attention, will find these consequences result naturally from them. Very little notice, however, was taken of my Letters, either in Dr. Hutton's second work, published in 1795, or in that of Mr. Playfair, which I shall more particularly keep in view, as having appeared in 1802, (after the last edition of my Letters to Professor Blumenbach) for the express purpose of illustrating and defending the theory of Dr. Hutton. In the discussion of that work will be seen the very slight attention bestowed by these two geologists on the *facts* which I had opposed to their opinions, and the truth of the remark made on that subject in the preceding pages. I had presented the greater part of the facts under the form of *generalizations;* and the same phenomena had also been generalized by Dr. Hutton; neither he nor Mr. Playfair were within reach of the places where my observations had been made; nor, doubtless, did they revisit those in which their own systems had been formed, in order that, on a fresh examination of them with a view to my generalizations, they might judge whether I was mis-

taken: that I was so, they have gratuitously assumed; and thus they have scarcely noticed my objections.

93. Yet this is not the way to promote the advancement of geology; a science which I have already shown to have become so interesting and so important to mankind. It rests on *facts;* which, however, every one has not opportunities of observing. According to the conclusions of Dr. Hutton, and of many other geologists, our continents are of indefinite antiquity, they have been peopled we know not how, and mankind are wholly unacquainted with their origin. According to my conclusions, drawn from the same source, that of *facts,* our continents are of such small antiquity, that the memory of the revolution which gave them birth must still be preserved among men; and thus we are led to seek in the book of Genesis the record of the history of the human race from its origin. Can any object of importance superior to this be found throughout the circle of natural science? Is there any one in regard to which we should be more cautious and attentive in investigating such facts, as may be adduced in opposition to our opinions? Facts, however, will ultimately prevail; we must therefore take care that they be not against us. For this reason, I have constantly made them my object; they will be found related in my travels; and I shall determine their respective classes, in the progress of such an examination of the system illustrated by Mr. Playfair, as I could wish him to have undertaken of mine.

94. The

94. The first object of my examination will be that part of his work, wherein he gives an exposition of the operations which take place on our continents, widely differing from that contained in my Letters to Dr. Hutton, though he does not himself take notice of the difference; but I shall point it out, as the instances of it shall successively occur. He enters upon the subject in § 96, and it is continued to § 103. I shall transcribe the whole passage, introducing my own remarks as I proceed. Our conclusions will be found to be diametrically opposite to each other, and both have however the same objects in view.

" It would far exceed the limits of this sketch," he says, " to pursue the *causes* of mineral *decompo-* " *sition* through all their forms. It is sufficient to " remark, that the consequence of so many minute, " but indefatigable *agents*, all working together, and " having gravity in their favour, is a system of uni- " versal *decay* and *degradation*, which may be traced " over the whole surface of the land, from the *moun-* " *tain top to the sea-shore*. That we may perceive " the full evidence of this *truth*, one of the most im- " portant in the natural history of the globe, we will " begin our survey from the latter of these stations, " and retire gradually toward the former.

" If the coast is bold and rocky, it speaks a lan- " guage easy to be interpreted. Its broken and ab- " rupt contour, the deep gulphs and salient promon- " tories by which it is indented, and the proportion " which these irregularities bear to the force of the

" waves,

"waves, combined with the inequality of hardness "in the rocks, prove that *the present line of the "shore has been determined by the action of the sea.* "The naked and precipitous cliffs which overhang "the deep, the rocks hollowed, perforated, as they "are farther advanced in the sea, and at last insu-"lated, lead to the same conclusion, and mark very "clearly so many different stages of decay. It "is true, we do not see the successive steps of "this progress exemplified in the states of the same "individual rock, but we see them clearly in different "*individuals;* and the *conviction* thus produced, "when the phenomena are sufficiently multiplied "and varied, is as irresistible, as if we saw the "changes actually effected in the moment of obser-"vation."

95. On reading these remarks, we might naturally suppose that they were designed as an answer to some system, in which it was pretended that the billows of the sea, in their attacks upon the rocky coasts, did not produce any degradation; and that it was sufficient to prove the fact of actual waste and decay, in order to show that the sea destroyed, and would continue to destroy, our continents. It was not, however, under this point of view, that I had presented the object in my Letters to Dr. Hutton. The question which I had examined was, whether "the action of the sea upon our coasts had deter-"mined their present line," and formed "the deep "gulphs and salient promontories by which they "are indented;" and such an opinion I had com-

bated,

bated, by producing facts altogether subversive of it. To all those who have studied the coasts, it is obvious that the "action of the sea" has, on the contrary, a direct tendency to *fill up the gulphs, and to destroy the promontories*, which existed around the continents, when the waters of the ocean were carried into their present bed; and I have shown that this action, so far from threatening our continents with destruction, must, by its very nature, at some future period *cease to operate*. This is what I had shown in my Letters to Dr. Hutton; and as no attempt has been made to remove this objection, it still remains in its full force. I am well acquainted with one of the coasts, of which the phenomena have been generalized by Mr. Playfair in the manner we have just seen; it is that of Devonshire and Cornwall; which, as he had elsewhere particularly mentioned it, I have visited, for the express purpose of verifying his observations; a caution that should always be used in similar cases, before we attempt to establish positively our own opinions. I have seen all the phenomena by which he had been struck; but he might with equal facility have perceived those, which, in consequence of an attentive observation of several other coasts, I had presented in my generalization; and which would have effaced the impreffion made upon him, by the former. The account of my travels on that coast will contain all these facts; and I shall then introduce his descriptions, from the 16th note to his work.

"On such shores," he here continues, "the frag-
"ments

"ments of rock once detached, become instruments "of further *destruction,* and make a part of the "powerful artillery, with which the ocean assails the "bulwarks of the land: they are impelled against "the rocks, from which they break off other frag-"ments, and the whole are thus ground against one "another; whatever be their hardness, they are re-"duced to gravel, the smooth surface and round "figure of which are the most certain proofs of a "*detritus,* which nothing can resist."

96. This is specious; and if we satisfy ourselves with this single view of these objects, we shall easily persuade ourselves, that, at the end of some millions of ages, our continents will be destroyed by the ocean. But if Mr. Playfair had observed in that manner, which he himself had before said, ought to be pursued, in order to acquire "a conviction as ir-"resistible as if we saw the changes actually effected "in the moment of observation;" if, for this purpose, he had attentively considered the "varied and multi-plied phenomena" concerned in that operation, he would have become sensible that the very "artille-ry," of which he speaks, leads to an approaching *termination* of the action of the sea upon the coast. It is not the sea which has produced the cliffs on the shores of our continents; and Mr. Playfair will him-self afford me an opportunity of proving it; they ex-isted at the birth of the continents. As long as the sea continues to be of a certain depth near the coasts, it can act upon them only by its waves; how there-fore is it enabled, in some places, to impel against

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them "fragments of rock?" It is because there the fragments, which had before fallen, having accummulated in the water at the foot of the rocks, are now arrived at the level of the sea. The beach thus formed, receives the new fallen fragments; and the waves assailing it with violence, continually propel the latest fragments towards the rocks, beneath which they form a slope, which may be seen at low water, and which, increasing gradually in height, is in time discernible at high water also. There indeed the "fragments are ground against one another" by the waves, and "reduced" to that "gravel," which is "impelled against the rocks;" but, in proportion as new fragments fall, the beach becomes more elevated, till at length the waves can no longer reach the foot of the cliffs, which by continual degradation are in time reduced into slopes. Of this process I shall, in my *Travels*, give many instances on the coasts to which Mr. Playfair refers; and having thus proved that the action of the sea really tends to destroy the cliffs, another passage of his work will furnish a direct proof that they could not have been originally produced by that action.

"Again," he continues, "where the sea-coast is "flat, we have abundant evidence of the degradation "of the land, in the beaches of sand and small gra- "vel; the sand banks and shoals that are continually "changing; the alluvial land at the mouths of the "rivers; the bars, that seem to oppose their discharge "into the sea, and the shallowness of the sea itself. "On such coasts the land usually seems to gain up-

"on

"on the sea, whereas, on shores of a bolder aspect, "it is the sea that generally appears to gain upon the "land. What the land acquires in extent, however, "it loses in elevation; and, whether its surface in-"crease or diminish, the depredations made on it "are in both cases evinced with equal certainty."

97. This exposition of the phenomena, which are observed where the coast is low, is perfectly correct; but at the same time it affords a proof that the action of the sea could not have produced steep and precipitous cliffs, where none had originally existed. The coasts, of which Mr. Playfair speaks above, are those which have a gradual declivity, extended to a considerable distance beneath the water. But "what the land" thus "acquires in extent" towards the sea, is not wholly at the expense of the higher grounds; the gain does not proceed solely from the sediments deposited by the rivers; it is, in a great measure, produced by the *sand of the sea itself*, which is impelled by the waves against the coast, and forms "sand-"banks," as well as "bars at the mouths of rivers;" as I shall show from the very coast on which Mr. Playfair has formed his generalization. All these phenomena, indeed, I had myself generalized only in my *Letters to Dr. Hutton*, in consequence of the observations which I had made on several coasts; and further I had pointed out the *final* effect to which such operations tend; namely, that when the sea becomes deeper close to the strand, which, at the same time, has gained in extent by the sand thrown up against it, an increase of declivity under the water is

is thus produced, and the action of the waves, in propelling the sand against that coast, will then *cease*. And, on the other hand, I had shown that rivers, when their beds shall be sufficiently levelled, straitened, and enlarged, will have but few materials to carry along with them to the ocean. Now such a statement Mr. Playfair should certainly have contradicted, had it been in his power; but he ought at least to have mentioned it, that his readers might have been enabled to form their own judgment on the question. For it is not only in direct opposition to the supposed destruction of the continents; but it evidently shows, from indisputable phenomena, that, if the borders of our continents had every where consisted of such low coasts, no action of the sea could ever have reduced them into cliffs; and that therefore the latter must have *existed at the birth of our continents*. Yet Mr. Playfair, as if, by his preceding arguments he had satisfactorily proved the tendency of the sea to destroy the continents, quits the coast, for the purpose of observing what passes "*inland*," where every thing, in his opinion, appears to concur in effecting their destruction.

"If we proceed," he says, "in our survey, from "the shores, *inland*, we meet at every step with the "fullest evidence of the same truths, and particular-"ly in the nature and economy of rivers. Every "river appears to consist of a main trunk, fed from "a variety of branches, each running in *a valley* "*proportioned to its size*, and all of them together "forming a system of vallies, communicating with one

"one another, and *having such a nice adjustment of* "*their declivities, that none of them join the prin-* "*cipal valley either on too high or too low a level;* "a circumstance which would be infinitely impro- "bable, *if each of these vallies were not the work* "*of the stream that flows in it.*"

97. *a.* Here Mr. Playfair is very correct in stating what phenomena *ought to be*, in order to make it appear probable that all vallies are the "*work of* "*the streams that flow in them;*" but, in supposing the real existence of such phenomena, he was not aware how completely he should himself afterwards overturn that whole system, when treating of another point, which will be presented in its place. Neither could he have had present to his mind the phenomena, which I had described in my Letters to Dr. Hutton, and which are well known to those who have attentively observed great mountains; viz. those numerous cascades that are still seen falling down on the sides of the principal vallies, the waters of which issue from *upper* ones, that join these at *too high* a level; and the multitude likewise of semi-cones, consisting of loose materials, resting on those sides, under the outlets of other vallies, of which also the level is *too high*, and where the torrents, running in channels too narrow for their volume, in abundant rains, have formed those cascades of rubbish, if they may be so called, by propelling downwards whatever materials they have collected in their previous course. In a word, nothing can be more contrary to *facts* than what Mr. Playfair here adduces as their generalization.

tion. This must be the result of very limited observations; and he has not thought of verifying the generalization which I have given of them in different works.

"If indeed" he continues, "a river consisted of "a single stream, without branches, running in a "strait valley, it might be supposed that some great "concussion, or some powerful torrent, had opened "at once the channel by which its waters are con-"ducted to the ocean; but, when the usual form of "a river is considered, the trunk divided into many "branches, which rise at a great distance from one "another, and these again subdivided into an infinity "of smaller ramifications, it becomes strongly im-"pressed upon the mind, that all these channels have "been *cut by the waters* themselves; that they have "been *slowly* dug out by *washing and erosion* of the "land; and that it is by *the repeated touches of the* "*same instrument*, that this curious assemblage of "*lines* has been engraved so deeply on the surface "of the ground."

98. This is indeed what Mr. Playfair had "strong-"ly impressed" upon his own mind; simply because he had not present to it that other part of his work, which I have already pointed out, and to which I shall hereafter come, where he speaks of *catastrophes* of the strata, not only capable of producing, but having actually produced, all the systems of vallies observable on our continents; a fact which I shall prove, when I enter upon an examination of that

part.

part. In the meantime, I shall make it appear, by what follows, that he has very superficially observed the phenomena on which he grounds his opinion.

"The changes," he says, "which have taken place "in the courses of rivers, are also to be traced, in "many instances, by successive *platforms of flat* "*alluvial land*, rising one above another, and mark-"ing the different *levels* on which the river has run "at different periods of time. Of these, the number "to be distinguished, in some instances, is not less "than four, or even five; and this *necessarily* carries "us back, like all the operations we are now treating "of, to an *antiquity extremely remote:* for, if it be "considered, that each change which the river makes "in its bed, *obliterates* at least a part of the monu-"ments of former changes, we shall be convinced, "that only a small part of the progression can leave "any distant memorial behind it, and that there is "no reason to think, that, in the part which we see, "*the beginning is included.*"

99. I shall have frequent occasion to remark, that Mr. Playfair endeavours silently to combat the arguments which, though he does not directly mention them, I had adduced in opposition to this theory, in my Letters to Dr. Hutton. Here, for instance, it is his aim to set aside those traces of a *beginning*, which I had pointed out, in the form of a generalization of the phenomena along "the courses of rivers," where I had introduced an observer following them step by step; and this he attempts, by endeavouring to prove that

all such traces must have been "*obliterated*," on account of supposed "successive platforms of flat "alluvial land," found, as he says, on the sides of vallies, at the bottom of which a river now flows; but I shall here proceed to show, first, that the supposition would be inadmissible, and secondly, that *no such platforms do in fact exist.*

100. According to Mr. Playfair, "it is by the repeated touches of the same instrument," that our vallies have been hollowed out. He there suggested the idea of a *saw*; and we shall soon find him expressly mention it. Now if the stream, by an operation analogous to that of a saw, had in effect lowered its own level, how could it have been able to *widen* the valley? Can a saw *enlarge* its way in the substance which it divides? In Mr. Playfair's opinion, the different levels, at which rivers have formerly remained, are ascertained by "alluvial land;" but alluvial land is formed only where rivers are able to *spread* themselves over tracts of some extent; and in these they make no *excavation*, but on the contrary, having lost their rapidity, they *deposit* such materials as they have collected in their course. These "platforms," therefore, cannot consist of "alluvial "land;" they afford no indication that the river has gradually sunk; and their true nature will be seen, by a comparison with real *alluvions*.

101. At the same time that rivers attack some parts of their shores, they carry along with them such materials as they detach thence, and deposit

them

them in places, over which they can spread themselves. It is thus that alluvial lands are formed; and, from the nature of their cause, if they contain gravel, it is found at the bottom; and if there is sand, it is at the surface, as having been the last sediment of the river, when it had ceased violently to attack its banks; and all these strata are horizontal. This operation I explained at large in my Letters to Dr. Hutton, and I gave an example of it in the new land formed by the Rhine near Coblentz; but similar instances may every where be seen. Now, in the platforms mentioned by Mr. Playfair, which I have often observed, the gravel is almost always at the *surface;* in places where it is covered by some layers of sand, a still greater quantity of sand is found beneath; and very frequently these different layers, which always preserve their parallelism to each other, are considerably *inclined,* sometimes in the direction of the declivity, but sometimes also in the opposite direction, and are *fractured* in the same manner as the stony strata. I shall have frequent occasion to remark this in my travels. The phenomenon is therefore very different from that of alluvial land; it belonged to our continents at their birth; and I shall take an occasion to explain the cause which produced it at the bottom of the sea. This is, indeed, a leading point in the investigation of *past causes,* which, in the theory under examination, is obscured by a *petitio principii,* repeated by Mr. Playfair, in his 16th note, p. 352, when quoting Dr. Hutton's *Theory of the Earth,* on a subject to which I shall hereafter come, namely, the *migration of stones.* "We see,"

he says, "the beginning of that long journey, by "which *heavy bodies* travel from the *summit of the* "*land* to the bottom of the *ocean*, and we remain "convinced, that, *on our continents, there is no spot* "*on which a river may not formerly have run.*" I trust that, when I enter upon that subject, I shall succeed in weakening this *conviction* in his own mind; but for the present I shall proceed to follow him in his exposition.

"In the same manner," he continues, "when a "river undermines its banks, it often discovers "*deposites of sand and gravel*, that have been made "when it ran on a *higher level* than it does at pre- "sent. In other instances, the same strata are seen "on both the banks, though the bed of the river is "now sunk deep between them, and perhaps holds "as *winding* a course through the solid rock, as if it "flowed along the surface; a proof that it must have "*begun to sink its bed,* when it ran through such "*loose materials* as opposed but a very inconsider- "able resistance to its stream. A river, of which "the course is both *serpentine*, and *deeply excavated* "in the rock, is among the phenomena, by which the "*slow waste* of the land, and also the *cause* of that "waste, are most directly pointed out."

102. On another occasion, Mr. Playfair loses sight of the latter part of this passage; but I shall revert to it in the proper place. I shall confine myself here to the examination of the first part, which relates to the strata of sand and gravel frequently found on the banks

banks of rivers, as well in places where their level is only *sometimes*, as in others where it is *always*, lower than that of the adjacent grounds. With an immediate reference to this phenomena, I shall proceed to trace the operations of rivers in *loose soils*; to these, indeed, we cannot too often return, for the purpose of describing new circumstances, as new questions occur; since it is an object respecting which so many errors have been committed by geologists.

103. A river may exhibit on its banks the sections of loose soils, either in the *new lands* which it has itself formed, or in *original grounds*; but the experienced observer may soon distinguish between the two cases. The rivers, which only sometimes run on a lower level than that of the adjacent grounds, are those that traverse their own alluvial land, and originate in a system of grounds, which, though very extensive, has proportionably few springs. After a long period of dry weather, rivers are fed exclusively by springs; and, when this happens, those, which rise in such systems as I have above-mentioned, run on a very low level in their several ramifications, particularly in their course through their alluvial land, although perhaps, in rainy weather, they overflow it. But this case may easily be distinguished; for the strata which the river, when very low, discovers on its banks, are as horizontal as the surface of the ground; and, if there be any difference in the size of the materials which are thus exposed to view, the largest, such as gravel, are found lowest, and those above consist of little else but sand. I had already mentioned

mentioned this circumstance, as a consequence of the operations carried on by rivers in other parts of their course since they had first begun to flow; operations to which I shall briefly return.

104. The places, where we see rivers running always on a level lower than the surface of the adjacent ground on both sides of their bed, are those where they have cut their channels, in *original soils;* when this is the case with respect to one side only, they have there encountered abrupt grounds: hence result three distinct phenomena. First, where a river has met in its course with a small eminence, formed of *loose materials,* round which it has not been able to wind its way, rising over it, and flowing down the opposite side at the most favourable point, its waters have loosened the soil, and gradually carried it away, continuing the work of excavation, as long as they have preserved sufficient rapidity to propel its materials towards some wider space. Such are the places, where the river runs in a channel more or less deep, and in which, as Mr. Playfair observes, "the same "strata are seen on both the banks;" but they are strata very different from those of alluvial land.

105. Secondly, where such an obstacle as the above had formed only a kind of *promontory,* round which the river had not been able to wind, without greatly bending its course, the stream has attacked that obstacle. It has then begun to straiten its channel, by cutting off the cape, and thus forming an abrupt *cliff;* at the foot of which it has continued to

flow, still causing it to retreat more and more; while on the opposite side, from which the stream has gradually withdrawn itself, it has left behind an horizontal space; which is a kind of *alluvial land*, easily distinguished from every other species, because it frequently contains materials of a considerable size, that have remained on the spot, while the river has propelled in its course such as were smaller. But as this sort of alluvial land is also liable to be overflowed, these large materials are now almost every where covered over with *sand*, or other loose substances, and are found only by digging through those soils, whence they are often taken for useful purposes. In this case, likewise, the section of the strata may be seen, but on *one side* only of the river.

106. Lastly, rivers have in some places encountered in their course cliffs of loose strata, which had *originally* existed on the continent, having been produced by the subsiding of the strata immediately before them, while they were still under the waters of the sea. An original cliff of this nature still discovers the section of its strata, if the river, flowing at its foot, and continually carrying away the materials which crumble down its sides, has not caused it to retreat so far, as that it may have been able to reduce itself into a *slope*, beyond the reach of the stream. This case too, so contrary to the notion that the rivers have excavated the vallies, is very discernible; for those *original cliffs*, with their slopes in various degrees of forwardness, are found on one of the sides of a stream, when there is not on the opposite side

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that *horizontal alluvial land,* which indicates the retreat of a river, in spots, where the cliff has been formed by cutting away a promontory encountered by the waters in their course.

106. *(a.)* Here then are three cases, in which the section of loose strata is shown on the banks of rivers, all absolutely distinct from that, where they flow in channels, which they have preserved through their *alluvial land;* and the attentive observer will not confound the latter with the former. The surface of the country renders the former immediately evident; it has none of the characters of alluvial land, as is obvious from its multiplied *inflections.* The strata, though parallel to each other, are seldom *horizontal;* and if their vertical sections sometimes present horizontal lines, the planes of the strata are, for the most part, *inclined* either backward or forward. Lastly, no order is visible in the superposition of the materials; beds of gravel occur, but they are generally at the *top,* or at a little depth below it; large stones are often met with, and even considerable blocks, of a nature very dissimilar to all the stony strata of the country; (an important phenomenon, which will hereafter engage much of our attention;) the rest of the strata frequently change, from sand, to marle or clay, alternately; and, as a final proof of their origin, *marine bodies* are often found in them. These details are surely of great importance for the intelligence of the channels of rivers. None of them, however, appear in Mr. Playfair's exposition, any more than remarks on what I had said concerning this subject

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in my Letters to Dr. Hutton: those remarks, it is true, were presented under the form of *generalizations*, similar to those which I have here offered; but they expressed distinct cases, examples of which will be found in my Travels.

" It is, however," continues Mr. Playfair, " when " rivers issue through narrow defiles among moun- " tains, that the *identity of the strata* on both sides " is most easily recognized, and remarked at the " same time with the greatest wonder. On observ- " ing the Patowmack, where it penetrates the ridge " of the Allegany mountains, or the Irtish, as it " issues from the defiles of Altai, there is no man, " however little addicted to geological speculations, " who does not immediately acknowledge, that the " *mountain* was once *continued* quite across the " space in which the river now flows; and, if he ven- " tures to reason concerning the *cause* of so wonder- " ful a change, he ascribes it to some great *convul-* " *sion* of nature, which has torn the mountain " asunder, and opened a passage for the waters. " It is only the *philosopher*, who has deeply medi- " tated on the *effects* which *action long continued* is " able to produce, and on the *simplicity* of the means " which nature employs in all her operations, who " sees in this nothing but the *gradual* working of a " *stream*, that once *flowed as high as the top of the* " *ridge, which it now so deeply intersects*, and has " *cut its course* through the rock, *in the same way*, " and almost with the same *instrument*, by which the " *lapidary* divides a *block of marble or granite*."

107. I am

107. I am not acquainted with the instances to which Mr. Playfair here refers; but he elsewhere cites others, which are known to me, and will occur in my Travels; and I am surprised that he has not in those been struck by the *dissimilarity* of the strata on the *opposite sections*, and in consequence induced to abandon a system thus grounded on their "iden-"tity." I shall not, however, dwell here upon this circumstance, but address myself to the "philoso-"pher," who imagines that he "has deeply medi-"tated;" or rather I shall introduce what would undoubtedly be the reasoning of a "lapidary," who should be told that a river had "cut its course through "a rock, in the same way" in which he "divides "a block of marble" with his saw.

108. The first circumstance which must strike him in this supposed analogy, is the assertion, that the "stream once flowed as high as the top of the ridge "which it now so deeply intersects." He very well comprehends that this ought to have been the case, had the ridge been cut through "in the same way" in which he "divides a block" with his saw. He effects this operation by placing his instrument on the stone, and drawing it alternately backward and forward; but a stream is in no respect analogous to a blade of steel; it has not the same inflexibility; nor has it any support, either to carry it to the summit, or in passing over it. He, therefore, sees clearly that neither the nature of his "instrument," nor the "way" in which he uses it for the purpose of "dividing a block of "marble," could have been considered, when his

operation was adduced as exemplifying in what manner a stream might have traversed the summit of a ridge of mountains, and "slowly" lowered its course by erosion, without having any support on either side of the ridge.

109. But our "lapidary" is thus led to "meditate" on that which engages the attention of the geologist. He has observed the *disorder* which prevails in the strata of the mountain, whence he detaches his blocks; and as he has found but few of any considerable size, on account of the broken and dislocated state of these strata, this circumstance leads him to make farther remarks. The mountain consists of several ridges, all intersected by a river, which, issuing through a defile, runs for some time in a valley, and then *bends* its course, to enter another defile. The face of one of the ridges, at the upper part, where the quarries lie, exposes to view the section of its strata: he knows that the strata *dip* considerably in the interior part of the mountain, and that, besides the defile through which the river traverses that ridge, different intersections of the summit are seen, on both sides of which the strata change their *level* and their *inclinations*. Another ridge, parallel to this, shows him the planes of its strata, much *inclined* towards the valley, of which the whole bottom is interspersed with small eminences of the same strata, there very much fractured and disordered. All these phenomena may undoubtedly suggest to him the idea of "convulsion;" not of such a partial kind, as only to have "torn the mountain asunder," but of

of those "convulsions" which gave birth to the mountains themselves, together with their defiles and their other accidents.

110. This again, no doubt, is a *generalization;* to which I have been led by considering what would naturally be the train of thought in the mind of a "lapidary," if a geologist had engaged him to reflect on objects, to which, long as they had been before his eyes, he had hitherto attended only so far as was immediately necessary for the working of his quarries. But similar phenomena will offer themselves in my *Travels*, under a variety of aspects, and even in places well known to Mr. Playfair. I trust, however, that, before I come to those direct proofs, I shall succed in convincing him that this is not a case where we need employ speculations, and have recourse to an "action" which had operated for an indefinite space of time; since he will himself soon indicate a *cause*, capable of *suddenly* producing *mountains*, where before there were only horizontal and continuous *strata;* and of forming those mountains into chains, composed of fractured strata, with those strong inclinations in every direction, which so evidently manifest the cause of the existence of mountains. There, indeed, many essential points of discussion will present themselves; but we must now continue to follow Mr. Playfair in his exposition.

"It is," he says, "highly interesting to trace up, "in this manner, the action of causes with which we "are *familiar*, to the production of effects, which as

seem

"seem to require the introduction of *unknown* and "*extraordinary* powers; and it is no less interesting "to observe, how skilfully nature has balanced the "action of all the *minute causes of waste*, and ren-"dered them conducive to the *general good*. Of this "we have a most remarkable instance, in the provi-"sion made for *preserving the soil, or the coat of* "*vegetable mould*, spread over the surface of the "earth. This coat, as it *consists of loose materials*, "is easily washed away by the rains, and is conti-"nually carried down by the rivers into the sea. "This effect is visible to every one; the earth is re-"moved not only in the form of sand and gravel, but "its finer particles suspended in the waters, tinge "those of some rivers continually, and those of all "occasionally, that is, when they are flooded or "swollen with rains. The quantity of earth thus "carried down, varies according to circumstances; "it has been computed, in some instances, that the "water of a river in a flood, contains earthy matter "suspended in it, amounting to more than the two "hundred and fiftieth part of its own bulk. The "soil, therefore, is continually diminished, its parts "being transported from higher to lower levels, "and finally delivered into the sea. But it is a fact, "that the soil, notwithstanding, remains the same in "quantity, or at least nearly the same, and must have "done so, ever since the earth was the receptacle of "animal or vegetable life. The soil, therefore, is "augmented from other causes, just as much, at an "average, as it is diminished by that now mentioned; "and this *augmentation* evidently can proceed from

"nothing

"nothing but the *constant and slow disintegration of* "*the rocks*. In the permanence, therefore, of a coat "of vegetable mould on the surface of the earth, we "have a demonstrative proof of the continual *de*-"*struction* of the rocks; and cannot but *admire* the "skill, with which the powers of the many chemical "and mechanical agents employed in this compli-"cated work, are so adjusted, as to make the *supply* "*and the waste of the soil* exactly equal to one an-"other."

111. Here Mr. Playfair has forgotten what he had said in his § 101, which, in my § 102, I referred to future notice. "In other instances," he had there asserted, "the same strata are seen on both the "banks, though the bed of the river is now sunk "deep between them, and perhaps holds as *winding* "a course through the solid rock, as if it flowed "along the surface; a proof that it must have *begun* "*to sink its bed*, when it ran through such *loose ma*-"*terials* as opposed but a very inconsiderable re-"sistance to its stream." The river, then, when it "*began*" to flow, found "*loose materials*" on the surface of the ground; now what is a coat of vegetable mould, but a "soil" composed of "loose mate-"rials?" And in what abundance, and to how great a depth, do we not find such soils on the greater part of our continents?

112. Such was the objection which I had already opposed, but more in detail, to this idea of Dr. Hutton, in the second of my Letters. Mr. Playfair, however,

ever, it seems, had as little noticed it, as he had remembered his own passage cited above; his object then was to explain in what way a river might have "a winding course through a solid rock," by the supposition that it had itself "sunk its bed" in loose strata; forgetting that he should soon have occasion to ascribe the *formation* of *loose strata* to the rivers. He would find it difficult, under this last point of view, to show the equal proportion, which he supposes between the "waste of the soil," occasioned by the transportation of its parts along the course of waters, and its "supply," proceeding from the "disintegration of the rocks" in those vast tracts of land, where no *rock* is discovered, at whatsoever depth the ground may be searched, whether upon the hills, or in the vales. In thus assigning a *final cause* to these pretended effects, Mr. Playfair has forgotten the precepts of Bacon, notwithstanding the high estimation in which he professes to hold them. That true philosopher strongly recommended to naturalists not to recur to final causes, till natural history and natural philosophy should be sufficiently advanced, to afford a well-grounded hope that the effects, now ascribed to certain causes, might not hereafter be shown to have no connection with them; for, when that happens, the scepticism of those, who deny the existence of final causes, is confirmed. I shall not then dwell on final causes, since we are now treating only of the immediate objects of natural history and natural philosophy, and consequently *physical causes* alone must engage our attention; but I shall allow myself to observe that the wisdom is more evident

dent of an arrangement, by which the continents, from the first moment of their existence, should have been supplied with a soil, *originally* adapted to the immediate purposes of vegetation, than of a plan, which would have required a considerable period of time after the birth of the continents, for the production of that soil; and in which the continual transportation of the materials of their surface to the sea is intended ultimately to "destroy" them, in order that, like the phœnix, new continents may arise from their ashes.

113. I have now followed Mr. Playfair in every thing that he has said relatively to the general operation of the *sea* and of *running waters* on our continents, since the period of their first existence; objects which, independently on their connection with Dr. Hutton's theory, and indeed with geology, are highly interesting with respect to natural history. It did not enter into Mr. Playfair's plan to examine my opinions on these objects, for he does not even refer to them; though it is by the discussion of different opinions that we arrive most easily at the truth; and this consideration has induced me to examine his system, in order thus to bring forward certain precise points, the decision of which must depend on the verification of the facts that relate to them; whence those facts may be more noticed, when presented in my travels. But several other points of importance in geology remain to be discussed, on which I shall continue to take Mr. Playfair's work as my text. A very essential one occurs in

in his § 112, where he reverts to his favourite hypothesis, that the *existence of vallies* is to be ascribed to the action of *running waters;* reproducing this opinion under such a form, as will for some time engage our attention.

"Many more arguments," he says, "all leading "to the same conclusion, may be deduced from the "general *facts*, known in the natural history of mountains; and, if the oreologist would trace back the "progress of *waste*, till he come in sight of that "*original structure*, of which the remains are still "so vast, he perceives an immense mass of *solid* "*rock*, naked and unshapely, as it first emerged from "the deep, and incomparably greater than all that "is now before him. The operation of *rains* and "*torrents*, modified by the hardness and tenacity of "the rock, has worked the whole into its present "form; has *hollowed out the vallies*, and gradually "*detached the mountains* from the general *mass*, "*cutting down their sides into steep precipices*, at one "place, and smoothing them into *gentle declivities* "at another. From this has resulted a *transportation of materials*, which, both for the *quantity* of "the whole, and the *magnitude* of the individual "*fragments*, must seem incredible to every one, who "has not learned to calculate the *effects of continued* "*action*, and to reflect, that *length of time* can convert accidental into *steady* causes. Hence *fragments of rock*, from the central *chain*, are found to "have travelled into distant *vallies, even where many* "*inferior ridges intervene:* hence *the granite of*

"*Mont*

"*Mont Blanc* is seen in the plains of *Lombardy*, or "on the sides of *Jura;* and the *ruins of the Carpa*-"*thian mountains* lie scattered over the shores of the "*Baltic.*"

114. This is a second instance, in which Mr. Playfair seems to have had an intention to obviate an objection of mine, without expressly mentioning it. The singular phenomenon of *blocks of granite* and other *primordial* "*fragments of rock*" being scattered in such abundance on some parts of the continents, so far from every *mountain* of their kind, is one of the most remarkable monuments of those *causes*, which, in former times, have acted on our globe; and no theory of the earth, which does not satisfactorily account for that phenomenon, can merit any confidence. When I wrote my first geological work, all the naturalists who had noticed these blocks had attributed their dispersion over the surface of the continents to the *action* of *running waters*. I combated this opinion by several arguments, and among others I adduced the following: viz. that *ridges* of *mountains* and *hills*, altogether foreign from the nature of these fragments, *intervene* between the places where they are dispersed, and every other mountain of their class. Mr. Playfair does not directly bring forward this objection, but he evidently has it in view, when he observes, that, owing to "the effects "of continued action," and to the operation of "time," which "can convert accidental into steady "causes, the granite of Mont Blanc is seen in the "plains of Lombardy, or on the sides of Jura, and "the

"the ruins of the Carpathian mountains lie scattered "over the shores of the Baltic," *notwithstanding* the "many inferior ridges" which "*intervene.*"

115. Those who are unacquainted with the Huttonian theory will not readily comprehend the possibility of such a migration; yet it is the only system in which it has been attempted, with any appearance of adequate means, to account for the transportation of these fragments of rock to the places where they are found at present, *notwithstanding* the ridges of mountains which "intervene." These means are indicated in the passage just cited; but many previous points must be considered, before we can proceed with advantage to the discussion of Mr. Playfair's explanation of that great phenomenon. To this system of "waste" occasioned by running waters, of which the vallies, more particularly, are imagined to be the effects, I had, in my Letters to Dr. Hutton, opposed the *lakes*, which are observed on the course of the principal rivers that issue from the Alps. Here then was a new impediment to the "transpor-"tation," not only of large "fragments of rock," but of every other kind of *detritus* of the mountains. Dr. Hutton had undertaken, in his second work, to remove this objection; and he has been supported in his attempt by Mr. Playfair. For the present, therefore, we must postpone the consideration of the blocks of granite, since this new subject will lead us into the most spacious field of monuments which the surface of the earth presents, wherein all known *physical causes* will be brought into action.

 116. Mr.

116. Mr. Playfair treats this subject in his sixteenth note; but again without mentioning the objections which I had made, and appearing only to speak generally of lakes, as of a phenomenon of the earth. He begins by indicating traces of the former existence of some lakes which, having been filled up by the gravel and alluvial earth brought down by rivers, are now converted into horizontal meadows, through which these rivers flow. That there are such meadows cannot be disputed; and although Mr. Playfair appears to me to exaggerate their number, I shall not dwell on this subject, because the lakes which still exist form the object of our present discussion. After having introduced the subject of lakes under this point of view, he thus proceeds, p. 357.

"From this gradual change of lakes into rivers, "it follows, that a lake is but a *temporary* and "*accidental* condition of a river, which is every day "approaching to its termination; and the truth of "this is attested, not only by the lakes that have "existed, but also by those that continue to exist. "*Where any considerable stream enters a lake, a* "*flat meadow is usually observed increasing from* "*year to year. The soil of this meadow is disposed* "*in horizontal strata: the meadow is terminated by* "*a marsh; which marsh is acquiring solidity, and* "*is soon to be converted into a meadow, as the mea-*"*dow will be into an arable field.* All this while the "sediment of the river makes its way slowly into "the lake, forming a mound or bank under the sur-

" face

"face of the water, with a pretty rapid slope toward "the lake. This mound increases by the addition "of new *earth*, *sand*, and *gravel*, poured in over the "slope; and thus the *progress* of *filling up* conti-"nually advances."

117. This is a very correct description of what takes place at the spot where a river flows into a lake; and Mr. Playfair refers us to that assemblage of lakes, which adorns the mountain scenery of Westmoreland and Cumberland, where he says that the progress of these operations may easily be traced. I shall have occasion hereafter to advert to a particular circumstance in this description; but, for the present, it is necessary to proceed to the point, to which Mr. Playfair desires to lead us, p. 359.

"The larger lakes," he says, "exemplify the same "progress. Where the *Rhone* enters the *lake of* "*Geneva*, the beach has been observed to receive an "*annual increase;* and the Portus Valesiæ, now "*Prevallais*, which is at present *half a league* from "the lake, was formerly close upon its *bank*. In-"deed, the sediments of the Rhone appear clearly "to have formed the valley through which it runs, "to a distance of about *three leagues* at least from "the place where the river now discharges itself into "the lake. *The ground there is perfectly horizontal* "*composed of sand and mud, little raised above the* "*level of the river, and full of marshes.* The dispo-"sition made by the Rhone after it enters the lake "is visible to the eye; and may be seen falling down "in clouds to the bottom."

118. Thi

118. This very essential point is introduced with great accuracy. The lake of Geneva was one of those which I had in view in my Letters to Dr. Hutton, and the mention of it was indispensable. The other instances adduced by Mr. Playfair are some lakes of North America, while at the same time he remarks, that the annual increase is there much less considerable than in those of the Alps; "as the rivers "which supply these vast reservoirs are none of them "very great." He then returns to his general subject.

"In order to give uniform declivities to the rivers, "the lakes must not only be filled up or drained, "but the cataract, wherever there is one, must be "worn away. The latter is an operation in all cases "visible. The stream, as it precipitates itself over "the rocks, hurries along with it, not only sand and "gravel, but occasionally large stones, which grind "and wear down the rock with a force proportioned "to their magnitude and acceleration."

119. Thus far I perfectly agree with Mr. Playfair; and I shall dwell for a moment on the consequence of this operation. It is certain, and I had myself remarked it, that when the rain waters began to assemble in torrents among mountains, they found in their course *abrupt sections of rocks*, whence they precipitated themselves in cataracts; as also *cavities*, which they must have filled up with such materials as they were carrying along with them, before they could convey farther any portion of these materials. This

indeed Mr. Playfair acknowledges; but here he forgets what I remarked (§ 97. *a.*) that he had founded his theory of the hollowing out of vallies, by the slow but long "continued action" of the running waters, on the circumstance that "all of them together form "a system of vallies communicating with one another, and having *such a nice adjustment of their "declivities, that none of them join the principal "valley, either on too high or too low a level.*" After quoting this passage, I observed that Mr. Playfair had very correctly stated what *ought to be the case*, in order to make it appear probable that "*all vallies "were the work of the streams that flowed in them;*" but that he had forgotten what *ought to be*, when he came to support his opinion by actual facts; for all the cataracts, which have fallen, and are still falling, down the sides of the principal vallies, manifestly show that some *lateral valley had joined them on too high a level*; and all the cavities, which are filled up by water, or alluvial earth, make it no less evident that *streams had there joined them on too low a level.* Here then we have *original inequalities* still existing in vallies, which running waters, far from having produced the vallies themselves, have had a tendency to efface: but, even supposing that these inequalities should have altogether vanished, these effects, from the traces left behind them, would be perfectly distinct from the *formation* of *vallies.* Mr. Playfair's opinion, therefore, that these inequalities are tending to be effaced, has no connection with the system that the vallies have been thus produced; and it is under this point of view that I shall examine the following passage, § 322.

"The

"The smooth surface of the rocks in all *waterfalls*,
" their rounded surface, and curious excavations,
" are the most satisfactory proofs of the *constant*
" *attrition* which they endure; and, where the rocks
" are deeply intersected, these marks often reach to
" a great height above the level on which the water
" now flows. The phenomena, in such instances,
" are among the *arguments* best calculated to re-
" move all *incredulity* respecting the *waste* which
" rivers have produced, and are continuing to pro-
" duce. They suffer no doubt to remain, that the
" *height* and *asperity* of every *waterfall* are conti-
" nually diminishing; that innumerable *cataracts*
" are entirely *obliterated*; that those which remain
" are verging toward the same end; and that the
" falls of Montmorenci and Niagara must ultimately
" disappear."

120. It is evident that this is quite unconnected with any proof that running waters have produced the vallies by erosion of the land; we are told only that "constant attrition" wears away the original "asperity" of the rock. But is this operation really carried on, in the manner described by Mr. Playfair? By no means; and he might have seen, in my second Letter to Dr. Hutton, that numerous waterfalls have been obliterated, and are tending to obliteration, not by the erosion of the rocks, but by the accumulation of the materials brought down by torrents, under the form of semi-cones. And with regard to waterfalls, which do not propel large fragments of stone, after they have once rounded the edge of the rock, they are

so far from *wearing it down*, that they flow only over the *moss*, which they permit to grow upon it; a case which I have observed in numerous instances, and particularly in the fall of the *Elbe*, near its source, in the Giant's Mountains. In the letter above-mentioned, I introduced the description given by M. RAMOND DE CHARBONNIERE of the fall of the *Adour* at Trames-Aigues, in the Pyrenees, which, as he observes, has " laid aside its ancient fury," and suffers *vegetation* to spread nearer and nearer to its course in every part. After this digression on the subject of waterfalls, which has evidently no favourable connection with the hypothesis of the production of vallies by running waters, Mr. Playfair returns to the *lakes*, and thus proceeds:

" Though there can be no doubt of the justness " of the preceding conclusions, when applied to " *lakes in general*, some *apparent exceptions occur*, " *in which the progress of draining and filling up* " *seems to have been suspended, or even to have gone* " *in a contrary direction.* These *exceptions* consist " of the lakes which appear to have *received a greater* " *quantity of materials than was sufficient to have* " *filled them up.* Such, for example, is the *lake of* " *Geneva*, which receives the *Rhone* descending " from the *Vallais*, one of the *deepest and longest* " *vallies* on the surface of the earth. Now, if this " valley, or even a large proportion of it, had been " *excavated* by the Rhone itself, as our *theory leads* " *us to suppose*, the lake ought to have been entirely " *filled up*; because the materials brought down by

" the

"the river seem to be *much greater* than the lake, "on any reasonable supposition concerning its original magnitude can possibly have received. What, "then, it may be said, has become of all that the "Rhone has brought down and deposited in it? "The lake, at this moment, retains, in some places, "the depth of more than 1000 feet; and yet, of all "that the Rhone carries into it, nothing but the *pure* "*water* issues. If it has been continuing to diminish, both in superficial extent, and in depth, from "the time when the Rhone began to run into it, what "must have been its *original dimensions?* I cannot "pretend to remove *entirely* the *difficulty* which is "here stated; yet I think the following remarks *may* "*go some length* in doing so."

121. We come at length to the critical point of the question, which, as far as relates to its nature, is very correctly stated by Mr. Playfair; although, in some respects, the "difficulty" is not brought forward in its full force, as I propose hereafter to show: but here a more essential remark presents itself. It is merely as an "*exception*" that he speaks of the lake of Geneva; it was, therefore, incumbent on him to prove in what way it formed an *exception* to every other phenomenon that was observed of the kind. He himself acknowledges that he "cannot pretend "to remove *entirely* the difficulty;" but is this a case, where we ought to rest satisfied with *attempts* alone? a case, where it is sufficient to make a few "remarks," which *may go some length* only towards removing the objection? This is an important question, on which I must

must dwell, before I proceed to notice the manner in which Mr. Playfair has attempted to obviate it.

122. In my first geological work, I had already opposed the actual existence of lakes to every theory of the earth, which assumed the formation of vallies by running waters; an hypothesis always found associated with some other, to which it is intended as a support. In itself, therefore, this forms a very important point in geological science; but in Dr. Hutton's theory, especially, it is a point of the highest consequence. His fundamental object is to prove that our *present continents are undergoing destruction, in order to supply the ocean with the materials of future continents.* This is the very "*law of* "*changes*," which, according to Mr. Playfair, should be the only object of the geologist. So strange a proposition ought to be established with all the evidence that facts are capable of affording, before geologists should submit to such a decision; and this we must always bear in mind, during our examination of Dr. Hutton's theory.

123. The only foundation then for this "law of "changes," as applied to an alternation of continents on our globe, is the hypothesis that these, which we now occupy, are suffering destruction, in order to supply the ocean with the materials of others which are to succeed them. For this purpose, it is *assumed* that all our present mountains were formerly *continuous masses,* of a much greater magnitude and elevation than they now possess; and that, by the

the action of running waters, they have been divided into ridges, between which are found the chasms called *vallies.* This, indeed, is the opinion of all those, who have ascribed the formation of vallies to a similar action; but in the Huttonian theory it is an absolute requisite; and we have already seen Mr. Playfair expressing himself to this effect, where he supposes that the materials, which are missing in those vacant spaces, have been gradually transported from higher to lower levels, and finally delivered into the sea, there to form those mineral strata, destined to become new continents. In my Letters to Dr. Hutton, I had opposed to the whole of that system the cavities of the *lakes*, which still exist along the courses of so many rivers, and more particularly on those of the principal streams which issue from the Alps; *cavities which must have been filled up, if the vallies in which rivers flow had really been excavated by them.*

124. I do not believe that a stronger and more peremptory objection can be adduced against any hypothesis; and it is evident that the theory must fall, unless this objection be no less peremptorily removed. Such a task was indispensably incumbent on Mr. Playfair; yet, in the passage last cited, we have seen him decline the attempt, and content himself, as if it were a point of very little consequence, with offering "remarks, which *may go some length*" only towards obviating the "difficulty:" to remove it *entirely* he acknowledges his own incompetency; and that his efforts, so far as they go, are in no degree successful,

successful, I shall now proceed to prove; not so much because I think it necessary, as because the objects, which will present themselves by the way, are of an interesting nature.

" *It is certain*," says Mr. Playfair, " *that from*
" *the present state of the lake of Geneva*, and of
" the ground round it, *we can hardly draw any in-*
" *ference as to its original dimensions.* Saussure
" has traced, with his usual skill, the marks of the
" course of the Rhone, on a level greatly *above* the
" present; and, by observations on the side of Mount
" Saleve, has found proofs of the *running of water*,
" at least 200 toises above the present superficies of
" the lake. But, if ever the superficies of the lake
" stood at this height, or at this height nearly, though
" we can conjecture but little concerning the state
" of the adjacent country, which no doubt was also
" on a higher level, the lake may very well be sup-
" posed to have been of far greater *dimensions* than
" it is now. It may have occupied the whole space
" from Jura to Saleve, and included the lake of
" Neufchatel; so that it may have been of *magni-*
" *tude sufficient to receive the spoils of the Vallais,*
" *which, as the surface of its waters lowered, may*
" *have been washed away, and carried down to the*
" *sea.* Thus it may have afforded a *temporary re-*
" *ceptacle for the debris of the Alps*, and may have
" served for an *entrepot*, as it were, where those
" debris were deposited, before they were carried to
" the place of their ultimate destination."

125. Such

125. Such is Mr. Playfair's first attempt to "re-"move the difficulty;" and his hypothesis appears to have some *facts* to support it; but at the very outset he has fallen into a mistake, because he is not so well acquainted as I am with the spots which he describes. "It is certain," he says, "that, from the "present state of the lake of Geneva, we can hardly "draw any inference as to its original dimensions." Now the contrary of this is "*certain*;" and he has himself introduced a circumstance, which will enable us to ascertain "its original dimensions" with the greatest precision. "The *sediments* of the Rhone," he had justly observed, "appear clearly to have "formed the *valley* through which it runs, to a dis-"tance of about three leagues at least from the "place where the river now discharges itself into the "lake. The ground there is perfectly *horizontal,* "*composed of sand and mud, little raised above the* "*level of the river, and full of marshes.*" The alluvial land of rivers, deposited at the entrance of lakes, points out, in the most irrefragable manner, the original level of their water, and very accurately determines their *first dimensions.* When the level of a lake has sunk, in consequence of spontaneous or artificial draining, the alluvial land, which had been deposited, is found to be *higher* than its surface; and Mr. Playfair adduces an instance of this in the alluvial land, which has divided a lake in Cumberland, and "is at present twelve or fifteen feet at least above "the level of the water" on each side; a circumstance which he ascribes to "a quantity of water of "that depth having been drawn off by the deepening "of

"of the issue at Ouse-Bridge." The level, therefore, of the Lake of Geneva has not sunk, since the alluvial land of the Rhone at its entrance is "*little* "*raised*" above its actual level. Its "original di-"mensions" are also at the same time determined, by the space which it at present occupies, including with it the above new land of the Rhone, and all such as have been formed by the other streams which discharge themselves into it; since nothing can be more distinct than the nature of those alluvial soils from that of every other ground in the vicinity of the lake, and since, like the lake, those soils are "ho-"rizontal."

126. Mr. Playfair was led to form the above hypothesis by a mistake of M. de Saussure, who imagined that he had perceived traces of a current "on "the side of Mount Salève, 200 toises above the su-"perficies of the lake." If what he observed were traces of a *current*, they were not those of the level of a *lake*, the waters of which have no perceptible current. Neither was it of the lake that M. de Saussure was speaking, but of a real *current*, of that which he called the *débacle*; by which expression he meant nothing less than the *retreat* of the *sea* from the surface of our continents, when it took possession of its present bed. But he was mistaken when he imagined that he saw any effects of that great current on the side of Mont Salève. I have often observed the excavations of which he speaks, and have perceived in them a very different phenomenon, frequently

frequently to be noticed in the abrupt faces of calcareous mountains, which I shall here describe.

127. The phenomenon, of which I am speaking, clearly indicates the *cause* of all abrupt faces of mountains, both on the sides of the vallies, and towards the plains, viz. *fractures* of the strata, succeeded by partial *subsidences* of the separated masses. The interval between Mont Jura and the opposite mountains, (one of which is *Salève*,) a tract of country in which is comprised the basin of the lake, was produced by a considerable subsidence; on each side of which the masses of strata, remaining on a higher level, formed those mountains, where they are now found in the greatest disorder. That mass of strata, in particular, of which Mont Salève is composed, is abrupt and precipitous where it fronts the lake, while the strata *descend* rapidly on the opposite side; it has, moreover, a fracture across its breadth, forming a dale, in which is situated a village called Monstier. By this fracture, Mont Salève is divided into two parts, which bear the names of *Great* and *Little Salève*. The strata of the latter are much inclined, in the direction of the length of the mass, towards the valley in which runs the *Arve*. Now, during the catastrophe of the great subsidence of the strata on the side of the lake, two strata, at different heights, on this face of Little Salève, having been fractured farther inward than the others, left *cavities* between them, in their fall towards that space; and thus were formed two galleries as it were, one above the other, which are called

called *Les Voûtes*. These galleries are what M. Saussure termed the *furrows* of the *débacle*. But besides that the cause which I have just assigned them is very visible, and that I have seen many of a similar nature on the abrupt faces of other calcareous mountains, these galleries have a gradual descent, in a direction opposite to that of the course held by the *débacle*, when it issued from the Alpine vallies.

128. Again, to suppose the level of this lake to have stood "200 toises above its present superficies," would answer but very imperfectly the end proposed; although, indeed, on that supposition, it would have comprised the three lakes of *Neuchatel*, *Morat*, and *Bienne*. For Mr. Playfair was mistaken, when, in order to calculate the quantity of materials, which the Rhone, if it had excavated its own channel, should have brought down into this space, he confined his attention to the valley where its main stream flows. He knows, indeed, that large rivers do not "consist of a single stream," but that they are "divided into *many branches*, which are again "subdivided into an infinity of smaller ramifications;" and, with me, he has considered all these as holding their course through a "system of vallies." Now, in this case, he might have applied to the whole "system" what he had said of the principal valley, that it is "one of the deepest and longest on "the surface of the earth." This, therefore, ought not to have escaped his attention. Moreover, the Rhone is no doubt the largest of the rivers which flow in that space; but it is not the only one, since many

others

others traverse it, issuing from the very large vallies of Jura and of the Alps; so that an elevation of "200 toises above its present level" would really bear but very little proportion to the increase of *vacancies* in that immense *system of vallies*.

129. Lastly, the notion of a "temporary receptacle for the debris of the Alps, which, as the lake's surface lowered, may have been washed away, and carried down to the sea," is perfectly inconsistent with what Mr. Playfair himself says of the nature of alluvial land. He first imagines the basin of the lake to have been filled up with these debris, to the height of 200 toises above its present level; now in what state, by his own account, would such a soil be? We have seen him thus describing it: "Where any considerable stream enters a lake, a *flat meadow* is usually observed increasing from year to year. The soil of this meadow is disposed in horizontal strata: the meadow is terminated by a *marsh*; which marsh is acquiring solidity, and is soon to be converted into a *meadow*, as the meadow will be into an *arable field*." Such, indeed, is the alluvial land of the Rhone, at the entrance of the lake. Supposing then this alluvial land to have been originally formed at a level more elevated by 200 toises, and, in the lapse of millions of years or ages, to have spread gradually, at the same height, so far as to have filled up the whole basin, what would have taken place, if the Rhone had afterwards lowered itself to its present level? That which always takes place, when rivers are forming their alluvial land,

land, if, by any spontaneous or artificial cause, their level sinks; it would have preserved, through the whole space, a channel proportioned to its volume; and, to use Mr. Playfair's own expression, the lake would have been "changed into a *river*," which then, in lowering its bed, would have simply formed *slopes* on its banks, similar to those which I have described; all the rest of the space would have continued in a fixed state, and the other streams which passed through it would in like manner have preserved their respective channels.

130. It thus appears that in all this display of *means*, for presenting the phenomena of the Lake of Geneva as an "*exception*" to what Mr. Playfair considers as a general case, there is nothing tending to "remove the difficulty," the reality of which he has himself acknowledged; the lake remains in every respect the same as other lakes, exhibiting *chronometers* at the mouths of all the rivers which enter it; and it should seem that he had particularly avoided all notice of that chronometer, which he himself could not refuse to admit, when describing the "horizontal ground, three leagues" in extent, commencing from the spot where the Rhone discharges its waters; and including the ancient Portus Valesiæ, which, having formerly stood on the margin of the lake, is now at "half a league's" distance from it. But Mr. Playfair himself has no great reliance on these means; and that for a reason, which he afterwards proceeds to mention, § 325.

"But

"But the great *depth*," he says, "which the lake "has at present, still remains *to be explained*, be"cause no mud or gravel could be carried beyond "the *gulf*, of a thousand feet deep, which was here "ready to receive it. *The reality of this difficulty "must be acknowledged; and some cause seems to act*, "if not in the *generation*, yet certainly in the *pre"servation* of lakes, *with which we are but little "acquainted*. We can indeed *imagine* some causes "of that kind to occur in the course of the *degrada"tion* of the land, which may produce new lakes, "or increase the dimensions of the old. The *wear"ing away of a stratum*, or body of *strata*, may "lay *bare*, and render accessible to the *water*, "some beds of mineral substances *soluble* in that "fluid. The district, for instance, in Cheshire, "which contains *rock-salt*, extends over a tract of "fourteen or fifteen miles, and is covered by a thick "stratum of clay, more or less indurated, which de"fends the salt from the water at the surface, and "preserves the whole mass in a state of dryness. "Should this covering be *broken open* by any natu"ral convulsion, or should it be *worn away*, as it "must be in the progress of the *general detritus*, the "water would gain admission to the *saline strata*, "would gradually *dissolve* them, and form of course "a very deep and extensive *lake*, where all was be"fore dry land. This event is not only possible, but "it should seem, that in the course of things it must "necessarily happen.

"Something of this kind may have taken place in

" the *track* of the Rhone, and may have produced
" the Leman Lake. It is not impossible, that, at a
" very *remote* period, the Rhone descended from the
" Alps without forming any lake, or at least any lake
" of which the remains are now *existing;* and this
" supposition, which is more *probable* than that of
" § 324, we shall soon find to be conformable to ap-
" pearances of another kind. The river may have
" *worn away* the secondary *limestone strata* over
" which it took its course after it left the schistus of
" the mountains; and, in doing so, may have reached
" some *stratum of a saline nature*, and this being
" *washed out*, may have left behind it a *lake*, which
" is but *modern* compared with *many* of the *revolu-*
" *tions* that have happened on the surface of the
" earth. (Note.—There are *salt springs* at Bex,
" near Aigle, about ten miles from the head of the
" lake: *saline strata*, therefore, are probably at no
" great distance.)

" This explanation is no doubt *hypothetical;* but
" it is proposed in one of those cases, in which hy-
" pothetical reasonings are *warranted by the strictest*
" *rules of philosophical investigation.* It is proposed
" in a *case*, where the *causes visible to man* seem in-
" adequate to the *effect*, and where we must there-
" fore have recourse to an *agent that is invisible.* If
" the operations ascribed to this agent are conform-
" able to the *analogy* of *nature*, it is all that can *in*
" *reason* be required."

131. And here I stop in the quotation; for Mr.
Playfair

Playfair, who could scarcely hope that this account of the phenomenon would be easily received, proceeds to make other attempts towards explaining the "difficulty," by which we shall be led to a new and very extensive field; and into that we cannot conveniently enter, till we shall have attentively examined his mode of conducting the present argument, which, he tells us, is "warranted by the strictest rules of "philosophical investigation." I feel no hesitation in taking Mr. Playfair himself for a judge, if he be willing for a moment to forget the interest which he naturally takes in his friend's theory; for I do not doubt that he will then see to what these "rules" really lead; surely not to *speculations*, when such *facts* may be consulted, as I am now about to state.

132. Dr. Hutton's theory, considered only in those points on which it agrees with many others, assumes that the ridges of mountains have been cut out from a mass, originally continuous; and that it is only by the slow erosion of running waters that the vallies, which separate these ridges, have been excavated. We have seen what Mr. Playfair has said on this subject. Such an operation might, perhaps, have required millions of years; but this is not considered as forming any objection, since the past is inexhaustible. To the above *hypothesis* I have not, therefore, opposed the length of time, but positive *facts;* by some of which I have shown that the waters could not produce the effect ascribed to them, and by others that they actually have not produced it. Among these facts, some are *decisive* in their kind; I mean

 the

the *lakes* observed on the course of the great rivers issuing from the mountains, phenomena which furnish two distinct arguments against this hypothesis. In the first place, if the vallies had been hollowed out by the waters which enter these lakes, the latter would no longer exist as such, but would be *filled up by the materials brought down into them*; and secondly, the materials really brought down by those waters being deposited in the lakes, their *whole quantity* is visible, together with that of their *increase* within a known period of time; and on comparing these two *quantities* with each other, it becomes evident that *not many ages* have elapsed, since the period at which the rivers, conveying these materials, began to flow.

133. Mr. Playfair himself remarks the progress of the alluvial land formed by the Rhone at the entrance of the Lake of Geneva; and when he would defend his hypothesis from the evident consequences of this fact, how does he attempt it? He has recourse to another hypothesis; supposing that this lake forms an "exception," and that its cavity has been produced by the dissolution of a "saline stratum." Unless I am much mistaken, a very different mode of proceeding should have been prescribed by the "rules of philosophical investigation," in a point of so much importance in geology. Before an *hypothetical* explanation of any phenomenon can be admitted, it is necessary first to prove the *existence* of that phenomenon, and then to show that it cannot be explained by any known *fact*. Now, with regard to the former point, the phenomenon here announced is itself

 only

only hypothetical; it is *supposed* that the Lake of Geneva forms an "exception" to all other lakes; whereas it was necessary to *prove* that it did in fact form an "exception." Instead, however, of attempting to demonstrate this, Mr. Playfair contents himself with simply referring us to his *theory*. Speaking of the valley of the Rhone, he says, "if "this valley, or even a large proportion of it, had "been *excavated* by the Rhone itself, *as our theory* "*leads us to suppose*, the lake ought to have been "entirely *filled up*.....I cannot pretend to remove *en*-"*tirely* the difficulty which is here stated." Then, after making one attempt to that effect, the consideration of the "great depth" of the lake brings him again to "acknowledge the reality of this difficulty," and to conjecture that "*some cause must act, with* "*which we are but little acquainted.*" But why should there be such a "*cause?*" Because his "*theory leads us to suppose*" that it is the Rhone which has "excavated the valley" in which it flows.

134. Such is the mode of reasoning adopted by Mr. Playfair: he does not prove the Lake of Geneva to be an "exception" to other lakes, which yet the true "rules of philosophical investigation" would have required; but he *supposes* it, because such an assumption was necessary to his theory. Thus he thinks it allowable to hazard the *conjecture* that there formerly existed a "saline stratum" on the spot, the dimensions of which were 22 leagues in length, 8 in its greatest breadth, and, in some places, from seven to eight hundred feet in depth; that it was subse-

 quently

quently to the dissolution of this mass that a deep and extensive lake was formed, an event "which is "but *modern*, compared with many of the revolu-"lutions that have happened on the surface of the "earth;" and that the Rhone has only since that event *begun* to accumulate the sediments found at the entrance of the lake; by which hypothesis we might indeed be enabled to trace back the commencement of that particular operation, but by no means to ascertain the epoch of the birth of our continents. This chronometer, then, is set aside, as applied to the latter period; but with as little reason as a person would have, who, in a debate respecting the time of the day, should object to some particular clock, under the pretence that it was an *exception*, and should attempt to assign a reason for its being too slow, while it was really keeping pace with all the other clocks of the country, and with the sun. This comparison is just; for, with regard to the *commencement* of the existence of our continents, the chronometer mentioned by Mr. Playfair himself, at the entrance of the Lake of Geneva, accords, not only with every one of those found at the entrance of other lakes, but with all the different kinds of chronometers which I have pointed out along the courses of rivers, and on the coasts, and in general with every effect which must have *begun* to take place on our continents at their *birth:* all these chronometers agreeing in their evidence that our continents have not existed during any *great number of ages*.

135. I am well assured that Mr. Playfair would not

not have indulged himself in such *conjectures*, if the country around Geneva had been as familiar to him as it is to me: for he acknowledges that he has recourse to an "*invisible agent*" for the production of this great cavity, only because he conceives that no "*cause visible to man*" is adequate to such an effect; now he could not have entertained this notion, had he been acquainted, not only with the mountains which surround the lake, but with the vallies which terminate there, and indeed with the whole assemblage of the vallies and lakes of Jura and of the Alps. I shall even venture to affirm, that, if he were unbiassed by the theory which he defends, the accurate descriptions of the mountains which encircle these lakes, and border the vallies, contained in M. de Saussure's *Voyages dans les Alpes*, (a work which is known to him, and on which he bestows a great and merited eulogium,) would have furnished him with "*visible*" signs of a "*cause*" that has equally produced both the *vallies* and the *cavities of lakes*. With the hypothesis that running waters have formed the vallies, Mr. Playfair cannot persuade himself that the latter and those cavities have a common "cause;" for running waters cannot excavate *below* the level of their course: according to his system, therefore, the cavities of lakes must be *posterior* to the existence of vallies, and must have been produced by a different "cause;" and if no one which is "visible to man" can be found for this purpose, recourse must be had to an "invisible agent." But I have shown, in some of my works, that the theory of vallies, which renders necessary

necessary the adoption of this arbitrary cause, is devoid of all foundation; yet the arguments which I have adduced on this subject have been overlooked by Mr. Playfair.

136. Here then is one of the most important questions in geology, to a deep discussion of which we are led by the opposite opinions entertained concerning it; nor can too much attention be bestowed upon an examination of it by *facts*, considering the length of time that has elapsed since the facts, which should have decided it, have been known. I am well acquainted with all the spots described by M de Saussure, and can bear testimony to the accuracy of his descriptions: I shall, therefore, make use of these alone, confining myself to the basins and immediate neighbourhood of seven lakes, situated at a small distance from each other, but crossed by rivers, which issue from different vallies. I shall begin with the circumstances which relate to the *Lake of Geneva*, and then proceed to those of the lakes of *Joux*, *Neuchatel*, *Bienne*, *Morat*, *Annecy*, and *Bourget*. To those, who are unacquainted with that interesting country, these descriptions will present, in one view, many great geological monuments, noticed in different parts of M. de Saussure's important travels; and they will, at the same time, more strongly engage the attention of those persons, who have already visited these spots, or may hereafter visit them.

137. In order to be thoroughly acquainted with the

the circumstances which respect the *Lake of Geneva*, it is necessary to take in, that whole space surrounded by mountains of which it occupies only a part. On the side where the *Rhone* enters, the lake is separated from the mountains solely by the alluvial land of the river; but this river, at the opposite extremity, where it flows out, traverses a considerable tract, consisting of small hills, on one of which stands *Geneva*. This tract lies between *Salève*, and the part of *Jura* which fronts that mountain; and the Rhone quits it through a defile of the mountains, called *L'Ecluse*. I shall begin the description of the vast circle which encompasses the lake with that which is given of this defile by M. de Saussure, § 313 of his travels.

"This defile," he says, "is the only one through "which the Rhone can issue out of the mountains.... "At this extremity, the *strata* of Jura are nearly "*perpendicular* to the horizon; for the most part, "they recede only 15° from the *vertical line*, and *dip* "to the eastward.....The strata of *Vuache* (on the "other side of the defile) have precisely the same "position."....I confine myself to the description of the course of the Rhone at its issue from the mountains, because I am not of the opinion expressed by M. de Saussure at the beginning of his work, that this issue has been cut by the Rhone itself. Such an hypothesis is useless, when we consider the great *disorder* of the strata, which, according to the above description, appears at this particular part of the passage of that river, and is observable along its whole course, and in all the mountains which border it,

it, as far as the well-known spot, called *La Perte du Rhône*. There its whole stream sinks through a fissure into a subterraneous cavern; but it again makes its appearance through many other fissures, flowing at first without any rapidity, amidst strata which are in the greatest disorder. When such symptoms of *convulsion* are visible, we are not surprised at seeing the Rhone precipitate its waters into the defile of L'Ecluse,

138. This extremity of the amphitheatre, closed by the *Mont de Sion*, which is a continuation of Vuache, offers nothing sufficiently interesting to require description. I shall pass on, therefore, to *Mont Salève*, against which the Mont de Sion rests, and with which commences that side of the enclosure opposite to Jura. The accurate description given of it by M. de Saussure begins § 232, and I shall insert that part which relates to our subject. " Besides the " large masses of strata, which constitute the body " of the mountain, and may be classed among the " horizontal, others are found, the position of which " is absolutely different. These are situated at the " foot of the *Great Salève*, on the side facing our " valley; where they lean against the inferior *sec-* " *tions* of the *horizontal strata*, and are either *per-* " *pendicular to the horizon, or highly inclined, rest-* " *ing against the mountain*.....These strata rise in " some places, for instance, between *Veiry* and *Cre-* " *vin*, nearly to half the height of Great Salève; " those in immediate contact with the mountain are " the most *inclined*. Some of the strata are *verti-*

cal,

"*cal*, and some even *inclined* in a *contrary* direc-
"tion, being supported by those which lie more out-
"wardly; these last form an angle with the horizon
"of from 60 to 65 degrees. These strata are in a
"great degree continuous, and extend to a consider-
"able distance. Their aggregate mass forms a thick
"rampart at the foot of the mountain; that they
"have been *fractured* is, however, evident; and
"in some places they are totally *lost*. This circum-
"stance is favourable to observation; because, by
"taking our station in the intervals, we can see their
"sections, and the whole of their structure."

139. I shall not transcribe the conjecture hazarded by M. de Saussure on the subject of these phenomena, which follows this accurate description of facts, more particularly as he afterwards changed his opinion. At the time when he wrote, he supposed that those vertical or highly-inclined strata might have been formed in their actual position; but we are indebted to himself for the first proof that all mineral strata were originally horizontal and continuous, and that their present state is to be ascribed to subsequent catastrophes. The mass of strata found overthrown at the foot of Salève, is the phenomenon of which I spoke, § 127, as proving that the plain intersected by hills between Salève and Jura, as well as the whole tract of country in which the lake is comprised, has been formed by a great *subsidence* of the strata; the basin of the lake being the part where the most considerable depression took place. This phenomenon will often be noticed in the descriptions which I shall

transcribe

transcribe from M. de Saussure; and it is frequently observable, not only in those sections of the mountains, which face the plains and the lakes, but likewise on the sides of the vallies: this similarity of *effects* proving the truth of my remarks with respect to the *identity* of the *cause*. The strata which rest thus, *vertically*, or with a considerable *inclination*, against abrupt sections of the strata of the mountains, indicate the place of the *fracture*; and those others which *dip* in the soil, or under the water of the lakes, are parts of the mass of the *depressed strata*, which have remained supported on the sides of the cavity, in which the subsidence took place. This will hereafter appear evident.

140. It would occupy too much time to follow M. de Saussure through the whole of his description of the tract of country of which I have been speaking; every part of it, however, is characteristic of the *cause* which I have indicated; but a full consideration of it would engage us in further details respecting *causes*; for we have here a theatre, exhibiting catastrophes after catastrophes, during the intervals of which, strata of *sandstone* were formed upon others of *calcareous stone*, which had previously subsided; and these afterwards together underwent new catastrophes, in which a considerable quantity of gravel, and of blocks of *granite* and other *primordial* stones, was disseminated over the whole of this scene of ruins. I shall, however, select some distinctive circumstances of these phenomena, beginning with the description of a mountain contiguous to Salève, but

but nearer to the lake; from which it is a league distant, though forming a part of that chain of mountains by which the lake is surrounded. The mountain of which I am speaking is called *Les Voirons*; the river *Arve*, issuing from the environs of *Mont Blanc*, passes between it and Salève, and not entering the lake, but pursuing its course at some distance from its extremity, it mingles its waters with those of the Rhone, which has already issued from the lake. The tract of country between the latter and Les Voirons consists of a plain, lying behind the hills which border the lake. The following is the description which M. de Saussure gives of this mountain, § 274.

"This mountain differs from Salève both in its na-
"ture and in its outward appearance. It is almost
"entirely composed of grit, of a greater or less de-
"gree of hardness.....Its strata are *inclined* towards
"the valley of *Boëge*, which separates *Les Voirons*
"from the chain of the Alps. The banks of Sa-
"lève (opposite the abrupt face) are *inclined* towards
"the same side; but the declivity of Les Voirons is
"more *rapid;* in many places, for instance, behind
"the ruins of the convent, it has an inclination of
"45°.....I have said that the strata of the mountain
"of Les Voirons are *almost* entirely composed of
"grit or sandstone; and I have expressed this re-
"servation on account of a large quantity of lime-
"stone, which lies near the southern extremity of
"the mountain, at nearly half its height, above the
"village of *Lussinge*. These strata of limestone are

"almost

"almost *perpendicular to the horizon*, in a direction "from east to west; the outermost strata are thin, "and intermixed with clay; but the innermost are "compact, and of considerable thickness."

141. Here then are two mountains very near to each other, which exhibit very different appearances, although the same strata are found in each; for beds of sandstone are likewise observable on the less inclined face of Salève, which looks towards the same side. And with regard to the strata of calcareous stone of Les Voirons, my brother, who is well acquainted with every part of that mountain, has traced the summits of their sections along its whole breadth; they rise up through the heaps of rubbish constituting that face of it which fronts the lake, where the declivity is gentle and gradual. On visiting this mountain together, we found by the barometer that its height, at the convent, near the summit, is 2800 feet above the level of the lake. The summit terminates in a sharp angle, formed by the section of a thick stratum of breccia, which is interposed between the calcareous strata, and the strata of sandstone. It is a very just remark of M. de Saussure, that beds of *breccia*, which separate two classes of strata, and of which the ground is of the same nature with the strata immediately *following*, are never found to contain any fragments, but such as belong to the strata which *precede*. Here, the fragments are of *primordial* and *calcareous stone;* none are of *sandstone;* for this began only to be formed on the calcareous stone of which its *first bed* involved

volved the debris, together with those of primordial stones. Now these calcareous debris have evidently belonged to the strata on which the sandstone was formed; for, in another visit, my brother found in those fragments enclosed in the breccia, marine bodies of a peculiar species, which are also contained in the calcareous strata projecting on the declivity; namely, palates of a kind of fish.

142. The *breccias* were the first phenomenon which led M. de Saussure to the interesting discovery that the strata, which are at present so much inclined as sometimes to be vertical, were yet formed in a *horizontal* position: for strata, which originally were so soft as to enclose fragments of other stones, could not, in any different position, have been formed of an equal thickness; their substance would have slidden to the bottom in one mass. All the strata, therefore, of Les Voirons, were once horizontal; a circumstance rendered equally evident by the remains of *marine animals* which they contain. The calcareous strata had, however, already undergone some catastrophes, being covered with their own debris; and such catastrophes had extended to the primordial strata over which they lay, the debris of these last being also found in the breccia; and this was their state, when the strata of sandstone were formed on them. After the consolidation of the latter, they, together with all the inferior strata, having been fractured in consequence of a fresh catastrophe, a large mass *sunk down* on the side next the lake; and another on the opposite side, towards which

those

those strata that retained the greatest elevation are now inclined; this is the cause of the singular form exhibited by Les Voirons. All doubts on that head would be removed on visiting the spot; and this mountain alone might serve to convince Mr. Playfair that the basin of the lake was produced by the *subsidence* of the strata.

143. I pass over other parts, also very characteristic of great catastrophes, and changes in the *nature* of the latest strata, which follow Les Voirons, in order to proceed to the consideration of greater phenomena of the same kind, described in § 320 of M. de Saussure's work.

"From Geneva to *Tour-ronde*, the eastern coast "of the lake is skirted by hills of sand-stone and "rounded gravel, and the mountains, properly so " called, are still at a considerable distance from its "banks. But beyond Tour-ronde, the mountains "are so close upon the lake, that it can be coasted "only by a narrow path, scarcely wide enough for a "horse to pass. Here, therefore, the lake, bordered "by lofty and *precipitous mountains*, no longer ex-" hibits to the view those smiling banks and beauti-"ful hills which skirt the other parts of its circum-"ference. Naked and barren rocks, or overhanging "forests, give it that wild and gloomy aspect, so "well described by the author of the *Nouvelle Hé-"loise.*"

144. The rocks here alluded to are the *Rochers de*

de Meillerie, which present their abrupt sections to the lake; they consist of strata of a blackish calcareous stone, shattered to such a degree as to be used only in walls composed of rough stones and mortar, or in laying the foundation of buildings. From its extreme hardness, it is well adapted to these purposes. The quarries are in the cliff; the stone is blown up by gunpowder, and the fragments fall, at the foot of the rock, on a kind of quay which is left there, whence they are shipped. The different depths of this lake, and of the others which I shall have occasion to mention, were measured by M. de Saussure, with a view to his interesting experiments on the temperature of the bottom of the lakes in the vicinity of the Alps; a temperature, which he found always to be lower, even in summer, than that of mines and deep cellars; an interesting object of natural philosophy, but one on which I cannot enter in this place. M. de Saussure was at first of opinion that the earth's temperature had been mistaken; but, having gone for the express purpose of making observations on the temperature of the sea off Nice, at the same depth as in the lake of Geneva, he found it to be there equal to that of deep cellars. One of my nephews, connecting the law of the dilatation of water by heat, which I have determined in my *Recherches sur les modifications de l'Atmosphère*, with the temperature of the surface of those lakes, in winter, has, in my opinion, discovered the cause of the constant low temperature of their bottom; but such a discussion would lead us too far from our subject.

145. I

145. I return, therefore, to that side of the lake, where, at some distance from the shore, M. de Saussure found the depth to be 950 feet. Speaking, in § 324, of the whole range of mountains, from Tourronde to *St. Gingough*, he says, "These mountains "are all calcareous; they are generally *precipitous* "on the sides which front the lake, and, in many "places, *strata, either vertical, or resting against* "*the bottom of the cliffs, are observable at their* "*base, as at the foot of Salève.*" If, taking our station on one of the projecting parts of those "pre-"cipitous" rocks, we observe the section of their dislocated strata which fronts the lake, and the detached masses, which, dipping in the lake, present only their summits to the view; considering that these descend below the surface of the water to the depth of 950 feet, as those at the foot of Salève descend to the plain, can we entertain any doubt respecting the identity of the phenomenon? Certainly not; and we are impressed with the idea that the existence of the lake is to be ascribed to the *depression* of the strata, which was *greater* there than in the other parts of that vast theatre of revolutions. The same conclusion will certainly be drawn from similar phenomena in the following descriptions.

146. That I may not lengthen them too much, I pass from St. Gingough to the neighbouring extremity of the lake, there to observe the space occupied by the alluvial land of the Rhone, which will present to us a phenomenon, similar to one described by M. de Dolomieu in the Delta of Egypt, where several calcareous

calcareous rocks rise above the alluvial land of the Nile; thus exhibiting masses of strata, which were at first in the sea near the coast, being the remains of those which had sunk, when the bed of the Mediterranean sea was formed. In like manner, there are two large calcareous rocks in the alluvial land of the Rhone, which, consequently, must at first have been in the water of the lake near this extremity. One of these rocks bears the name of *Champigny*, and the other of St. *Tryphon*. M. de Saussure describes them § 1092. On measuring the height of the latter by the barometer, he found its elevation to be 246 feet; and the description which he gives of both, evidently points to a depression of strata, the disorder of which is manifested by the state of those masses that remain exposed to sight. "These "rocks," he says, "are not however of the *same* "*kind*, and much less of the *same structure:* for "that of St. Tryphon is composed of strata, regular "and *horizontal*, or nearly so; while those of Cham-"pigny are highly *inclined*, and frequently much *dis*-"*ordered*." Were these rocks, it may be asked, in the "saline stratum," which Mr. Playfair supposes to have been there dissolved? If he had seen those spots, his hypothesis would never have occurred to him. But let us attend to M. de Saussure, who gives us, in § 1095, a general idea of the mountains embracing this extremity of the lake.

"The range of calcareous mountains, from *St.* "*Maurice* to *Chillon*, scarcely ever exhibits regular "and horizontal strata; nearly all of them are *dis*-

"*located* and *inclined*, and appear to have been sub-
"jected to the action of *violent causes;* for, in my
"estimation, *depressions* alone do not seem sufficient
"to account for the variety of their forms. There is
"much irregularity also in the positions of their
"*abrupt sides;* most of which, however, are turned
"towards the valley of the Rhone. That range of
"mountains, which corresponds with these, on the
"left bank of the Rhone and of the lake, is likewise
"calcareous, and almost equally *irregular*. Most
"of these mountains, such, especially, as are nearest
"to the lake, are very *steep*, both on the side of the
"lake, and on that of the Rhone: the vallies, by
"which they are separated, appear to divide them
"into chains parallel to the lake, which stretch in a
"direction from N. E. to S. W. The mountains
"nearest to the lake, as I have just remarked, *pre-
"sent their abrupt sections towards it*, while those
"lying at a greater distance from the lake, and near-
"est to the centre of the Alps, are inclined *towards
"the latter*. The *Val de Lie* separates these two
"chains of mountains: this rich and fertile valley is
"in the form of a *cradle;* the two chains which bor-
"der it rise up on each side in a gentle *acclivity*, and
"their opposite *abrupt faces* are respectively turned
"towards the lake and towards the Alps."

145. *a*. Such minute and precise descriptions, which, if any doubts were entertained of their accuracy, might be verified on the spots themselves, ought to restrain the imagination, disposed as it ever is to create new hypotheses. Here assuredly are pointed out

out to us "*visible*" indications of the "*cause*" which has operated in the production of *vallies*, and of their deepest hollows, the *cavities of lakes*. The "*abrupt* sides," exhibiting "sections" of the strata, are the parts where the *fractures* have taken place, while the void space before these shows a complete subsidence of the mass thus separated; a subsidence generally characterised by portions of that mass remaining elevated above the heaps of ruins, such as the rocks of St. Tryphon and Champigny. This indicates, as M. de Saussure well observes, *more than a simple depression or subsidence;* some "*violent cause*" must have attended it, by which the broken masses were shaken during their fall. Such a *cause* will hereafter be manifested by other symptoms; but all the facts connected with it must first be assembled in our view. In other parts, where any fracture has occurred, the strata are found simply *inclined* towards the point where it took place, either on both its sides, or on one only. The first case is exemplified in the above instance of the Val de Lie, where the strata ascend on both sides, turning their abrupt sections outwards, towards the lake, and towards the Alps; and examples of the second case may be found in many vallies, one side of which exhibits the abrupt section of the strata, while their inclined planes form the other side. On beholding such and so common phenomena, how is it possible to maintain that running waters have produced the vallies? Is not their real cause in a manner "*visible?*"

146. *a.* The part of this lake which is opposite to

 the

the Alps, and lies under Mont Jura, is very interesting to the geologist who, already convinced that vallies and the cavities of lakes are the necessary effects of successive subsidences of the strata, is desirous to trace the formation of *new strata* after each catastrophe, and to ascertain what fresh revolutions have taken place, subsequently to the production of the latter; an object, in regard to which much still remains to be observed, and which requires the multiplication of observations and local descriptions. But those who enter on this task will do well previously to examine the proofs which I shall adduce, that their inquiries ought really to be directed to the effects of depressions frequently repeated, and to the formation of new strata, in certain places, during the interval of such catastrophes. I shall therefore omit the interesting description given by M. de Saussure, of that side of the lake, where, between its banks and the precipitous rocks of Jura, the landscape is adorned with those fertile hills, of which there are none on the side of the Alps, and which, on account of the variety of their species, and the *disorder* so prevalent in their strata, are the phenomena which I have pointed out, as eminently worthy of study. But here I must confine myself to the great features of the catastrophes, as manifesting their general nature; for which reason I pass on to other scenes, described in the 16th chapter of the first volume, entitled, *Les Lacs du Jura.*

147. The first of these lakes, of which M. de Saussure is about to give us a description, will lead

us quite away from that great *current*, which, according to Mr. Playfair, after having undermined the schisti and the calcareous strata, might have dissolved a " saline" mass. We are now on Mont *Jura*, at a level of nearly 2000 fect above that of the lake of Geneva, in the *Vallée des Rousses*, in which is the lake of that name, the highest in those mountains, and about three quarters of a league in length. The small river issuing from it, called the *Orbe*, forms afterwards three other lakes, the principal of which is the *Lac de Joux*, elevated 1900 feet above the lake of Geneva; it is about two leagues in length, and half a league in breadth; the mountains lie so close to it on one side, that it cannot there be coasted; and in that part is its greatest depth, which M. de Saussure found to be 80 feet. The second lake, contiguous to the former, is the *Lac de Brenel*, which is about a league in circumference; and the third, of smaller dimensions, but of considerable depth, is called the *Lacter*, a corruption, as is supposed, of *Lacus tertius*. The division of these lakes is occasioned by the disorder of the strata at the bottom of the valley, respecting which I shall now enter into some details.

148. The *Dent de Vaulion* is a summit elevated 1440 feet above the lake of Joux; M. de Saussure, speaking of this eminence, as affording an instance of the convulsions which the strata in that part of Mont Jura have undergone, says, § 343, " It is thus " that above the source of the Orbe" (the second source, which will hereafter be noticed) " the Dent " de

" de Vaulion exhibits to the *northward* the *abrupt* " *section of its highly inclined strata;* while on the " other side of the valley, opposite to the Dent, the " strata of another chain of mountains *rise abruptly* " towards the *south.*" And in § 381, " the strata " of the Dent de Vaulion descend on the side next " the Alps, being *inclined* in angles of from 30 to " 40 degrees; and they are *cut vertically* on the side " next the valley of the Orbe, above which they form " a *frightful precipice.*" Its depth is 2120 feet.

149. The manner in which these lakes are drained is likewise worthy of the greatest attention; the following is the account given by M. de Saussure of that remarkable phenomenon. " Nature has pro- " vided for the discharge of the waters of these " lakes, by preparing for them *subterraneous pas-* " *sages,* in which they are ingulfed, and lose them- " selves. But it is not through wide channels or " large apertures that the waters penetrate into the " earth; it is *through the interstices of the vertical* " *strata of calcareous stone,* which compose the " mountains surrounding these lakes, and particu- " larly that of Brenel, towards the west and north. " .....The most considerable of these *tunnels* is the " work of nature; but it has been much improved " by art. It is situated to the N. E. on the bank " of the small lake, at about the middle of its length, " in a hollow of a mountain of some height, which " at that part is close upon the lake, and of which " the strata are exactly *perpendicular to the horizon.* " As the waters rush into this kind of abyss with " considerable

"considerable violence, mills have been erected over
"their stream, below the level of the lake, which
"are called the *Moulins de Bon port*. A strong
"dike keeps in the waters of the lake, and a conve-
"nient supply may at all times be procured, by
"means of the openings, which have been made in
"this embankment, and which are properly managed
"by a sluice." Before we consider further the spot where this water is thus lost, let us proceed to that where the same river, which had entered the lake of Joux, makes its appearance in *Val-orbe*, at the place usually called its source, swollen by the waters which enter the three lakes by lateral streams.

150. The following description is given by M. de Saussure of this very remarkable spot. "'A rock of
"a semi-circular form, at least 200 feet in height,
"consisting of large *horizontal* strata, cut down
"*perpendicularly*, and intersected by rows of firs,
"which grow on the cornices formed by the sa-
"lient parts, *terminates* to the westward the valley
"called *Val-orbe*. Mountains of greater elevation,
"and covered with forests, enclose this rock on all
"sides, except where they leave a passage to the
"Orbe, of which the source is at *the foot of the*
"*rock*. Its waters, which are perfectly *limpid*, flow
"at first with *majestic calmness* over a bed of green
"moss, (fontinalis antipyretica) but soon the main
"current, carried down a *rapid declivity*, *foams*
"among rocks occupying the middle of its channel;
"while the waters near the banks, less agitated, and
"continuing to glide over a *verdant bed*, form a con-
trast

"trast to the white foam in the middle of the stream. "And thus does the river again escape from our "sight, running through a deep glen, overgrown "with firs, of which the gloom is rendered more "striking by the lively verdure of the beech trees "interspersed among them. On beholding this "spring, we can readily imagine how poets have "been led to deify fountains, or to convert them "into the residences of their Gods.....The perpen- "dicular height of the surface of the lake of Joux "above this source of the Orbe was found on mea- "surement to be 680 feet." The river afterwards, pursuing its course, flows into the lake of Neuchatel, near Yverdun.

151. This is one of the phenomena, by which, at an early period, I was convinced that vallies had not been excavated by running waters, and was led to fix my attention on mountains. I observed it in 1743, having crossed those mountains as far as *Pontarlier;* after a short interval I crossed also the Alps by *St. Bernard,* and the Apennines by *La Bochetta;* and though I was then but young, the aspect of the vallies of these mountains, and of their summits, struck me very forcibly. Since that time, I have observed a variety of phenomena, similar in their *kind* to this second source of the Orbe; and the differences obvious in their *species* have guarded me against deducing general hypotheses from particular phenomena. If the deep chasm in Val-orbe, which terminates against the mountain, or any other of the same *kind,* were exclusively to engage our attention, we might be induced

ced to suppose it the effect of some great *earth-ake;* a conjecture which I have indeed heard made specting similar phenomena; but no such conjectures would be hazarded, were the concomitant circumstances studied with care. I shall make this vident, in the present instance, by introducing in the orm of queries the remarks which the case might aturally suggest, and by giving such solutions as re furnished by the circumstances which accompany t.

152. I. Why does not the Orbe, after having run it so considerable an elevation in the upper valley, our down its waters in a *cataract* into the deep chasm of Val-orbe, resembling the many other waterfalls seen in similar situations? Because, at the extremity of its course in the upper valley, it meets, though between mountains, with *vertical strata,* extending up to the mountains themselves, through the interstices of which its waters continually filtrate. The chasm therefore of Val-orbe cannot have been occasioned by an *earthquake,* on continents already existing; for that could not have produced the *angular motion* of such a mass, in the centre of the mountains, so as to have rendered *vertical,* strata, which had before been *horizontal.*

153. II. Since the waters thus find subterraneous passages, why do they accumulate on the mountain in such a degree as to form these lakes? Because they cannot meet with sufficient openings for their whole volume, without spreading themselves so far

as

as to embrace a greater number of fissures; and they can effect only by rising. The inhabitants the valley are even obliged to take care that the w of the lakes find sufficient passages to carry off superfluity; since otherwise they would overflow villages situated on their banks. Speaking of precautions, M. de Saussure says, "They sink "from 15 to 20 feet in depth, and from 8 to "breadth, in the *thin and vertical strata*, of "the tops are even with the ground, on the b "of the small lake. The waters discharge t "selves into these pits through channels mad "their conveyance, and thence they pass by inf "tion into the interstices of the strata. Thes "the pits usually called *tunnels;* and from tin "time they are cleared of the mud and slime "choak them up."

154. III. Can we possibly imagine that Orbe, in the upper part of its course, has *vated the valley of those lakes?* Assuredly for the current of the river is there scarcely pe tible. Entirely *quiescent* in its passage throug Lac des Rousses, it preserves the same tranq in the Lac de Joux; the space which it trav from the latter to the Lac de Brenel, is extr short, and the Lacter is rather a large pond th lake, although it is of considerable depth. No sible *erosion*, therefore, can have taken place in valley, and yet its sides are of great elevation. it is commanded, among other rocks, by the De Vaulion.

155.

155. IV. Why are the waters always clear and "*limpid*" in Val-orbe, even in rainy seasons, when those which run into the lakes above from the surrounding mountains are constantly *turbid?* We are acquainted with no "*visible cause*" which can account for this phenomenon; we must, therefore, according to Mr. Playfair's rule, here truly applicable, have recourse to an "*invisible*" one; and that can be no other than a *cavern*, into which the infiltrated waters penetrating, deposite their slime. The proof of this is furnished by another circumstance. The usual limpidity of the waters of the Orbe, as they issue at Val-orbe, had led many to doubt of their identity with those, which, at the Lac de Brenel, pass away by infiltration; but all doubt on the subject is removed by the following fact, recorded by M. de Saussure. "In the preceding years, the "waters having risen to a higher level than was convenient to the inhabitants of the Vallée de Joux, "they resolved, in 1776, to repair and clean out the "*tunnels* of the Lac de Brenel. In the hopes of "draining it, they shut up, by a strong dike, the "canal, through which the great lake maintains a "communication with it; but, when the waters were "raised to a certain level on one side, and had become proportionably lower on the other, their "pressure was so great as suddenly to occasion the "bursting of the embankment. An excessive agitation of the waters ensued; they became *turbid* "throughout; and soon afterwards the Orbe, in "Val-orbe, which till then had been perfectly clear, "appeared *thick and muddy* at its issue." This accident,

cident, no doubt, was productive of a good effect, and passages were then opened, which before had been obstructed. The waters, which then rushed into the *cavern*, were in too great abundance, and formed too rapid a stream, to allow of their becoming "*limpid*" by depositing their slime; and in consequence they came out in the state described. There are many other phenomena in these mountains to prove that large *caverns* are concealed in them, which can have been produced only by the unequal *subsidence* of the strata in the same masses, during their catastrophes.

156. This single spot, attentively studied, has already led us to draw several important conclusions: there still, however, remains an object for examination, namely, the "*moss*," over which the waters of the Orbe "flow with majestic calmness," when issuing from beneath a rock composed of *horizontal strata*, at the foot of great masses of *vertical strata*; (a difference of position which accounts for the *cavities* found in those mountains.) At the point of its issue *begins* a waterfall, similar to many others which I have seen; for it is of little consequence whether, after having been "carried down a rapid declivity, "and foaming among the rocks which occupy the "middle of their channel," the waters pursue their course, or whether they fall precipitously from a rocky summit into a valley beneath; to observe them at the point of their *descent* is what is really important, to enable us to judge whether *cataracts*, as Mr. Playfair supposes, can form any evident parts of a

general

general system of waste and degradation. Here the waters, having entered the subterraneous cavern, are retained there at a certain level by an embankment. If, in rushing out, they had been capable of cutting the dike by erosion, their level in the cavern would have sunk: but we find the external edge of that dike constantly covered with that species of moss, which consists of long filaments. I have observed this in many places, at the *edge* of rocks, from which torrents fall, and which they certainly do not injure in their course; since, in many places, there is room to pass between the sheet of descending water and the rock, which is overgrown with *moss*, and other plants, usually found in damp caves. Even the great cataract of the Rhine at *Schaffhausen*, exhibits the same appearance; the stream majestically pursues its *unruffled* course over the *brow* of the rock, and begins to "*foam*" only at its foot, in consequence of the torrent dashing itself against its own waters.

157. Before I quit this important phenomenon, which so clearly manifests the state of the strata in the mountains, and how unnecessary it is to seek in running waters the cause of the existence of the vallies through which they flow, or to call in the aid of any "invisible agent" to account for the cavities of lakes, it may be proper to state, that neither *subterraneous passages* for the waters through the interstices of the strata, whether vertical, or highly inclined, nor *caverns*, whence they issue forth in some low valley collected together in a "calm and limpid"

stream, are by any means unusual phenomena; and that cases similar to these are in particular observable in many parts of Jura. The only circumstance, therefore, more remarkable in the phenomenon just described than in others, is that of the upper lakes, occasioned solely by the cavities which the waters *found* there; and in which it was necessary that they should attain a certain elevation, before they could spread themselves so far as to reach a sufficient number of openings into the interstices of the strata; whereas, on other rocky heights, the rain waters immediately filtrate, even through the turf, into the interstices of those vertical strata, the sections of which form the summits. Sometimes, indeed, if there happens to be a more favourable fissure at the bottom of a declivity, rows of small tunnels are met with in the turf. All these waters being afterwards collected together in caverns, form rivers, which issue peaceably through some outlets in the sides of low vallies; down which they must have precipitated themselves in cataracts, had they not *found* such internal passages. The above is the case with the *Reuse*, which, like the Orbe, discharges its waters into the *lake of Neuchatel;* it is formed in *Val-travers*, which terminates abruptly against the mountain, but has many recesses, like that of Val-orbe whence issue several rivulets, already formed; and these, uniting, become the Reuse.

158. Those who love the study of nature, and more particularly those who are aware how far geological science may lead us in the road to her sanctuary,

tuary, will not consider the foregoing details as having been extended to a tedious and unnecessary length. The very circumstance of my being engaged in the present discussion sufficiently proves that *generalizations of phenomena*, however exact they may really be, have no influence on those, who imagine that they also have accurately *generalized* the same phenomena; and that the *details* alone can produce the desired effect, when they have been observed in places, where the descriptions may be verified, if any doubts are entertained respecting their correctness: and thus, likewise, may many persons be induced to fix their attention upon objects, which are within their reach, but from which it had not occurred to them that any instruction could be derived. I shall therefore now proceed, still acompanying M. de Saussure, on the road to the lake of Neuchatel, setting out from the banks of the lake of Geneva, which he describes in § 390.

"From *Balaigre*," he says, "we came in three "hours and a half to *Yverdun*, where we passed "the night.....On the road, we had several very fine "views of the lake. We were struck, upon first be-"holding it, with the great extent which it must "formerly have had; for the spacious *marshy and* "*horizontal* meadows by which it is terminated on "the S. W. have unquestionably been at one time "covered with its waters. We shall have occasion "to make the same remark on the other side of the "lake." This is the side which may be considered as the entrance of the lake; for its waters flow

out

out at the opposite extremity: it is on the side of *Yverdun* that the river *Orbe* runs into it, together with other streams. The alluvial land here mentioned continues to increase, but now very slowly; the first deposites, on the contrary, accumulated rapidly, (as in the case of all alluvial grounds,) from the abundance of debris which the waters found in their *original channels*, and from their tendency to straiten their course.

" In going to *Susseve*, which is at the distance of " a league and a quarter from Yverdun, I remarked," proceeds M. de Saussure, " banks of grit, or soft " sandstone, *rising in a slope* towards Jura. A " quarter of a league farther on.... commence strata " of yellow calcareous stone.....From Yverdun we " went to *Neuchatel*, where we slept; the distance " between these towns is only seven leagues.....The " lake is eight leagues in length, and two in breadth. " ....On its banks, as well as on those of the lake of " Geneva, are found rounded pebbles of all sizes, " together with considerable blocks of granite, and " other Alpine stones; many of these occur between " Yverdun and *Grandson*. This lake lies much " nearer Jura than that of Geneva, particularly at " its northern extremity, where it waters the lowest " strata of those mountains. The road from Yver- " dun to Neuchatel in many places passes over these " strata; for the most part they *ascend* towards the " body of the mountain; some of the strata however, " above *Vaumurcus*, are almost *perpendicular* to " the horizon, and have not a *direction parallel* to

" that

" that of Jura. M. De Luc, in taking the mean of " two observations with the barometer, has fixed the " elevation of the lake of Neuchatel at 26 and a " half toises above that of the lake of Geneva. M. " Pictet determines this elevation at about 31 toises; " and as he has made five different observations, " which correspond with each other, the latter result " seems entitled to the most confidence.....The next " day we went to sound the depth of the lake.....At " half a league's distance from the bank, we found a " depth of 325 feet." I have already observed, that, in his visits to these lakes, M. de Saussure had principally in view his observations on the temperature of their bottoms; he has therefore contented himself with exhibiting some features only of the surrounding mountains: his descriptions are all correct, but I shall now and then have occasion to notice some circumstances, which he has passed over, beginning at the *lake of Bienne*, whither we will now accompany him.

" We quitted Neuchatel on the following evening, " and passed the night at *Cerlier*, which stands upon " the margin of the *lake of Bienne*. We left that " place at a very early hour, for the purpose of re- " peating in this lake our observations on the tempe- " rature of deep water. The lake of Bienne, like " that of Neuchatel, is situated immediately at the " foot of the first chain of Jura. These two lakes " are separated only by plains, probably at one time " covered by their waters, which were then united. " The lake of Bienne is about three leagues in length,

"and nearly one in breadth. At a league and a "half from Cerlier, we found its depth to be 217 "feet.....During the time that the thermometer was "under water, we returned towards the island of "*St. Pierre*, a delightful spot, and highly worthy "of the fame which it has acquired. This island "lay on our left, at a distance of three quarters of a "league from Cerlier.....Its length is about a quarter "of a league, and its breadth a little less. It is a "hill of an irregular form, the highest point of which, "according to a barometrical observation of M. Pic- "tet, is elevated 121 feet above the level of the lake; "and the lake itself is 178 feet higher than that of "Geneva." This confirms M. Pictet's observation on the height of the lake of Neuchatel: for a difference of nine feet in the level appears to me to be sufficient between these lakes, which are justly considered by M. de Saussure as having formerly constituted but one: in the intermediate space there remains only a canal, by which the waters of the lake of Neuchatel discharge themselves into that of Bienne.

"The hill of the island has a gentle declivity to- "wards the south, where it terminates in a small "plain.....From thence, on the western coast of the "lake, are seen the extensive vineyards at the foot "of Jura. The eastern coast of this lake forms a "striking contrast with the western; its steep and "elevated borders present only *naked and rugged* "*rocks*, rising boldly from the edge of the water, or "forests, above which the highest summits of the

"Alps

Alps are alone visible.....The soil of the island, of which the *vertical section* is seen near the most elevated point....exhibits, beneath the vegetable mould, first sand, then a soft clay, next an indurated and coloured clay, and lastly strata of sandstone, not very hard, and of a fine grain, which are worked as quarries.....On our return to Cerlier, we immediately proceeded to *Morat*, which is three leagues from it. We traversed the *marshes*, at the northern extremity of the lake bearing the 'name of the latter town, where we with difficulty 'extricated ourselves from the boggy land. These "great *horizontal* marshes, little raised above the "level of the lake, formed probably at one time a "part of its bed; and then the waters of the three "lakes of *Neuchatel*, *Morat*, and *Bienne*, were "all collected in one basin."

159. These are all the details into which M. de Saussure has entered relatively to the environs of these lakes, and they may serve to point out their principal distinguishing features: to his descriptions, however, I shall subjoin some further particulars. According to every appearance, the waters of these three lakes must have been formerly united, though their depth was inconsiderable in the part now occupied by the horizontal soils. Some alluvial land has been produced by streams, in the intervals between the lakes; but much less, I apprehend, than their own waves have accumulated. Besides, even at the time when these three lakes probably occupied only one basin, the parts, which now form those of

 Neuchatel

Neuchatel and Morat, were separated by a ridge of low hills, presenting their steep sections to the lake of Neuchatel; in the same manner as those on the eastern bank of the lake of Bienne, and also the abrupt face of the more elevated part of the island of St. Pierre, present theirs to the latter lake. It is therefore only by the excavations being larger, and filled with water, that these parts are distinguished from many others in the same position at the foot of mountains, in vallies lying between small hills; the latter consisting of ruins of the fractured strata; which, in the general subsidence, have remained above the level of the rest.

160. With respect to *visible marks* of a considerable subsidence, there is no spot which exhibits them more evidently than this. Nearly the whole base of this part of Jura consists of strata *leaning* against the foot of the mountains, of which those described by M. de Saussure, from Grandson to Neuchatel are a part. But the most considerable phenomena of this kind are observable in the interval between the lakes of Neuchatel and Bienne, and along the western bank of the latter, where, towards the bottom of the declivity, and on the rubbish, are the vineyards mentioned by M. de Saussure. That interval exhibits a phenomenon, very remarkable both from its nature and its cause. This consists of several distinct masses of these inclined strata, which have evidently formed a part of those that show their abrupt sections in the upper part of the mountain behind them. These masses ascend in the form of steps;

steps; those on the outside being the lowest, and those which follow rising higher and higher against the side of the mountain; and their thickness is so considerable, that a glen, more or less wide, is formed between the section of one mass, and the inclined plane of the following. Now in the cause, by which the separation of these distinct masses in their fall was occasioned, we shall find one of the examples of a phenomenon, which, in my first letter to Dr. Hutton, I had opposed to his hypothesis respecting the consolidation of our strata by extreme heat; namely, the interposition of strata retaining their *softness*, between *indurated* strata. In the instance before us, very thick strata of *soft marle* lay between others of *calcareous stone;* these two kinds of strata naturally separated in their fall; and thus the masses, originally the most elevated in the mountains, fell forwards to the bottom of the caverns, into which sunk down all that had occupied the space of the present lakes; the succeeding masses sunk less, being stopped in their descent by the former; the next, still less; and thus were formed those gigantic steps, which I have just mentioned. Marine bodies are found, both in the strata of marle, and in those of calcareous stone; but they are not all of the same class; I found, for instance, some very beautiful mother-of-pearl Nautili in a stratum of the former; but I perceived none in the calcareous stone.

161. It is not many years since that great stock of marle was discovered, by which the products of agriculture in the country of Neuchatel have been

so

so considerably augmented. In the breaking and lateral fall of the mass of strata in that place, those of marle, which had lain between others of calcareous stone, detached themselves from the strata which they had covered, and followed the strata which had rested upon them. Now those strata of calcareous stone being of considerable thickness, their shattered section, which was turned towards the glen, separating one mass from the next above it, became subject to degradation; and from its debris was formed a slope, by which the section of the strata of marle was concealed. This circumstance having been discovered by the inhabitants, they have found it easy to clear away the rubbish of these slopes, and are come to the marle, which they dig out from beneath the strata of calcareous stone. That marle, which is of a bluish cast, becomes friable, and crumbles, after having been for some time exposed to the air; it is then spread over the land, which, by this means, is made to yield abundance of the grass called *espacette*, (saint foin, *hedysarum onobrychis*.) The fodder being thus increased, an increase of cattle naturally follows; and the manure from them is highly favourable to the growth of vines. Formerly the marle was not found in such abundance; it was met with only accidentally under small eminences, on the outward declivities of the mountain, and in the vallies; those eminences being fragments detached from these very strata, during the catastrophes by which the vallies and the basins of the lakes were produced.

162. In the space which separates the lake of Nèuchatel from that of Bienne, where, as I have said, those masses of strata, ascending against the mountain in the form of immense steps, are principally observed, the lowest mass dips under the soil; but this range stretching, with occasional interruptions, along all the western coast of the lake of Bienne, the strata there dip under the waters of the lake, by the edge of which there has been therefore, in some places, great difficulty to make a road. Now all along this bank, and even in the lake itself, is found a great quantity of blocks of granite, as is the case in the other vallies and on the summits of Jura. Can it be doubted, on beholding these monuments of catastrophes which I have here assembled, that the whole tract of land, which lies in front of Jura, has been reduced to its present level by a subsidence of part of the strata originally belonging to those which now form that mountain, the *basins* of the lakes being the parts where the *subsidence* was the greatest; when, at the same time, we evidently see the place of the fracture, in the abrupt section of the upper part of the mountain? These strata, dipping into the lake, may be compared to finger-posts, placed at the entrance of roads; for they plainly point out to us where those masses sunk, which are missing in front of the mountain.

163. Let us now attend M. de Saussure to another lake, which lies near the first chain of the Alps, in the same manner as the preceding lakes lie in the vicinity of the first chain of Jura. This lake is

is situated in a country abounding in great geological monuments, which will detain us for some time. I shall here collect their principal features from different parts of M. de Saussure's *Voyages dans les Alpes*, adding to them occasionally my own observations. The lake, of which I am now speaking, is that of *Annecy;* and it is thus described by M. de Saussure, § 1162.

"This lake is about four leagues in length, and
"one league in its greatest breadth; its general di-
"rection is from north to south. It is surrounded
"on every side by *high mountains*, excepting near
"*Annecy;* where the mountains connected with the
"chain of the Alps terminate, and a series of de-
"tached hills commences. All these mountains are
"calcareous. Towards the middle of the lake there
"is an island, joined by a causeway to the main
"land. This island bears the name of *Chateau-*
"*vieux*, and is sufficiently large to contain a castle,
"several gardens, and some fine orchards. The
"situation is truly romantic; and the diversified
"prospects offered by it of the deep and pellucid
"waters of this small lake, and of the *abrupt moun-*
"*tains* by which it is surrounded, although some-
"what wild and gloomy, are interesting and pictu-
"resque.....The height of this lake, on taking a mean
"between M. Pictet's observations and my own, is
"35 toises above the lake of Geneva.... Its greatest
"depth, at a place called *Boubio*, half a league to
"the S. W. of the town, is 180 feet."

164. Here

164. Here again M. de Saussure, whose immediate object was to ascertain the temperature of the bottom of the lake, has omitted to give any particular description of the surrounding mountains; but he elsewhere describes other parts of the same chain. For my own part, I have only a general recollection of the chaotic state of their strata. Towards the other extremity of the lake, there is a passage in the mountains, which leads to the valley of the *Ysere* in the *Tarentaise;* and near the upper entrance of this passage, called the *Col de Tamier*, stands a convent, the situation of which, amidst mountains of so ruinous an aspect, is well adapted to its order, being that of La Trappe.

165. The mountains which surround the lake of Annecy, separate it, by an interval of four or five leagues, from the valley of *Faucigny*, whence issues the *Arve*, proceeding from the region of Mont Blanc. They form the termination of one of the most considerable branches from the central chain of the Alps, and are called *Les Bornes:* they are of great height; on one side of them is the recess, in which lies the lake of Annecy; between their other extremity, which is named *Mont Brezon*, and the mountain *Mole*, is the entrance of the great valley of Faucigny. The face of Les Bornes, which is everywhere precipitous, is divided into many distinct eminences; it fronts that side of Salève, of which the strata are *inclined* towards it, the *abrupt face* of the latter mountain being on the opposite side. These spots, to which our attention is now to be

be turned, are all indicated in the chart of the lake of Geneva, and of the adjacent mountains, which is prefixed to the *Voyages dans les Alpes* of M. de Saussure.

166. The broad valley, which lies between the *lofty and abrupt* face of *Les Bornes*, and the comparatively *low and inclined* side of *Salève*, is one of the spots which I could wish to point out to those, who imagine that running waters have hollowed out the vallies, and who must therefore suppose, with Mr. Playfair, " that each branch of a river runs in " a valley proportioned to its size," (p. 102.) " and " that the *same strata* are seen on both *sides*," (p. 104.) With regard to this last opinion, although the strata are calcareous, both of Les Bornes and of Salève, they yet differ greatly in many respects. Those of Les Bornes, belonging, like those of Meillerie, to the calcareous strata of the external chains of the Alps on that side, are of a brownish grey; while those of Salève, which belongs to the chains of Jura, are generally, like the latter, of a yellowish white. As to the valley itself, although it is very broad, there run through it only a few scattered rivulets, which proceed immediately from Les Bornes and Salève, and flow into the Arve; for this part of the valley gradually rises, as far as Annecy, where another declivity begins in the opposite direction; the waters on that side discharging themselves into the Rhone, much below where it receives the Arve. Such a partition of waters indicates one of those boundaries of systems, which I mentioned in my third

third letter to Dr. Hutton. This division of waters, like many others, is situated in a valley; and it is evident that its place cannot sensibly have changed, since the birth of our continents, as the streams, being only *nascent*, are there, at the *minimum* of their action. I shall now proceed in M. de Saussure's descriptions, § 442.

" Opposite *Bonne-ville* and the Mole, on the other " side of the Arve, is a calcareous mountain, called " the *Brezon*. I have twice or thrice ascended its " summit. The most elevated of its rocks are quite " *perpendicular;* they rise to a very great height " towards Bonne-ville, and form a frightful preci- " pice. In order to view it without danger, I lay " down on the rock, and brought myself forward by " degrees, till my head projected beyond the brink " of the precipice. By taking this precaution, we " may accustom ourselves to look down without fear " or giddiness into abysses of the greatest depth."

167. This may serve to give an idea of the *form* of a great many of the mountains in those chains. The section here has been on the side next the Arve, and that river, although it flows at a considerable distance, might yet be supposed, in the course of millions of years, to have cut this passage between the Brezon and the Mole; but while the side of the latter mountain, nearest to the Arve, descends towards the stream in a *slope*, its opposite side has an abrupt section, similar to that of the Brezon, though no great stream is near it. I was obliged to use M.

de Saussure's precaution in looking down this latter precipice, which is no less frightful than that, of which I have given his description above; it being vertically cut from the summit to the depth of at least a thousand feet; its basis consisting only of an heap of broken masses of the depressed strata, which masses, when viewed from the opposite valley, exhibit the appearance of hills. The strata of the Mole descend so rapidly from this summit towards the Arve, that it would be almost impossible to ascend the mountain, which is covered with grass, were it not for the transverse sheep-tracks. When I arrived at the summit, I found it so sharp, that I could sit on it astride, having one leg on the side of the declivity, and the other suspended over the precipice. In this posture however, which was only a frolic of youth, I did not venture to look down, but laid myself, for that purpose, along the slope, and gradually advanced my head over the brink. Two principal *fractures*, among others, have produced the form of this mountain; from one of them, attended with a great subsidence of a mass of strata, has resulted that part of the valley of *Faucigny*, towards which the whole mass of those which form the Mole is inclined. The other fracture having happened on the opposite side of the same mountain, and being attended with a similar subsidence, a valley has been produced there also, but with the heap of ruins which I have mentioned. This last fracture has formed a curve, in the concavity of which is the sharp ridge of the precipice; but on the side of the Mole which fronts the lake of Geneva, part of the

mass

mass of strata has remained; so that, when viewed from thence, the mountain appears in the form of an obelisk. I measured its height by the barometer at the highest point, which I found to be 4560 feet above the level of the lake of Geneva. The following is the description which M. de Saussure gives of the entrance into the valley of Faucigny, §443.

" On leaving Bonne-ville, we crossed the Arve by " a long and narrow bridge, and entered a valley, to " which all the great characters of the Alpine val- " lies belong. The entrance into it is flanked by " two lofty mountains, the Mole to the north, and " the Brezon to the south, bearing the appearance of " two fortresses destined for its defence. The bot- " tom of this valley is perfectly *horizontal*, and " covered with *marshy meadows*, being watered by " the Arve and its tributary rivulets.....Its direction " is nearly easterly; its length, from Bonne-ville to " Cluse, is about three leagues; and its breadth, at " the entrance, is scarcely half a league: it *widens* " afterwards, but again *contracts*, as it approaches " Cluse, where it almost entirely closes." Do we here recognize the "*work of the stream*," or the "*repeated touches of the same instrument*," which denote, according to Mr. Playfair, that the existence of vallies is to be ascribed to the waters that run in them?

" Wherever the ground is opened, the bottom " consists of sand, disposed in *horizontal strata*, " and sometimes placed alternately with layers of

rounded

"rounded stones and gravel. The nature of the "soil, and the perfect level of the surface of the "valley, make it evident that this bottom has been "formed by the accumulation of the deposites of "the Arve; and that this river, or the current occupy-"ing its place, has formerly run at a much higher level "than it does at present, since it must have filled "the whole of the valley, of which it now occu-"pies so very small a part." In this first volume, M. de Saussure advances several opinions, which subsequent observations have since induced him to relinquish. The current no doubt levelled the bottom of the valley, and must consequently have pervaded the whole of it; but this could have taken place at different times only, and while the waters were still employed in bringing down the quantity of rubbish, which they found in their upper channels: they then held a vagrant course, among their own deposites, as is still the case with some rivers: but since they have almost ceased to bring down with them any other materials than dust and small gravel, and since the valley has been inhabited, it has become easy to restrain the course of the stream, in order to improve those parts of the bed of the valley, which so far from being lowered, have been raised by the accession of the alluvial soil; a circumstance, which, in all the inferior vallies, has universally taken place.

"The road leading to *Cluse* is very beautiful; "for the space of a league it forms a rectilineal "and horizontal causeway; but afterwards, the Arve "shaping

shaping its course towards the mountains on the right, the road passes over the debris which are accumulated at the feet of the rocks. These debris are for the most part calcareous; they nevertheless are mixed with granite and other primary stones, transported thither by the same revolutions, which have dispersed similar fragments in the environs of Geneva; for the adjacent mountains are all calcareous, and the primary are still very distant. .....After having proceeded about three quarters of a league along the foot of the mountain, we again descended into the horizontal valley. We passed through the large village of *Siongy*, where the *Chartreux du Reposoir*, to whom it belongs, have a house, which is easily recognized, from its being the best in the place. The road leading from Siongy to Cluse, a distance of half a league, is extremely pleasant; this valley, like that of Taninge, produces the finest oaks in the country. On the left of this small plain, an ancient castle, erected on the summit of an *insulated rock*, the foot of which is covered with trees, forms an interesting object in this very delightful and picturesque landscape.

" With regard to the structure of the mountains which border this valley, and their correspondence with each other, I shall first observe that, although the Mole and the Brezon, placed on the opposite sides of its entrance, are of the same height, and are both calcareous, there is yet no other resemblance between them. In the colour and quality

of

" of the stone, the general form, the structure and
" position of the strata, they are altogether different.
" The dissimilarity is even greater between the other
" mountains which enclose this valley; nor do the
" *salient* and *retreating* angles in any degree *corre-*
" *spond.* But let us enter into some details, and
" first consider the mountains on our right hand; we
" will next proceed to those on the left.

" I have already remarked that Mont Brezon,
" which fronts Bonne-ville, has its summit on that
" side *vertically cut.* Its strata first obliquely *incline*
" *backward,* or to the S. E.; and in proportion as
" they extend in the direction of the valley, their
" inclination changes, their declivity increases, and
" at last they *incline forward,* or towards the east.
" But the foot of this mountain is also, like that of
" Salève, covered with large masses of strata nearly
" *perpendicular* to the horizon, *leaning* against the
" body of the mountain; and although the Brezon
" itself terminates at the distance of half a league
" from Bonne-ville, yet those strata, which *lean*
" against the foot of the southern chain, and thus
" turn their back to the Arve, continue to stretch as
" far as the village of Siongy, a distance of nearly
" two leagues. They are interrupted indeed by a
" small valley, at the farther extremity of the foot of
" the Brezon, but beyond that vacant space they again
" appear.

" This small valley, which opens at the foot of
" the Brezon, is *narrow* and *winding; salient* angles,
" opposite

"opposite to *retreating*, are there plainly discern-
"ible. It leads to the village of *Brezon*, situated
"behind the mountain of that name."

168. The difference between these two remarks of M. de Saussure, relative to the *salient* and *retreating* angles, gives me occasion to observe that such a cor respondence frequently takes place in *narrow vallies*; these being commonly the effect of a *single fracture* of the strata, with some inclination backward of one of the sides, or of both; but unattended by the subsidence of any intermediate mass below the level of the bottom. These meetings of salient and retreating angles, both often very acute, being generally characteristic of a fracture, may be much more naturally assigned to that as their cause, than to the serpentine windings of streams, a cause first imagined by M. BOURGUET. With respect to the chasms of *large vallies*, their form consists of all those irregular widenings and contractions described by M. de Saussure, they having been produced by *two fractures*, and the subsidence of the intermediate part, attended with many other catastrophes; such as the fractures which form the lateral narrow and winding vallies; and during which those masses of strata, detached from the sides, were stopped at a certain height, as on the border of the basins of lakes, in their fall towards the part depressed. To these we may also add, as well the islands in lakes, as the insulated rocks in vallies, which are fragments of the broken strata, remaining like ruins, elevated above the rest; and it is only the rubbish, brought down by the first

currents from the upper vallies, which prevents our perceiving the original disorderly state of the bottom of those cavities, it being now rendered level by alluvial ground. Narrow and winding vallies are for the most part, as I have said, single fractures; however in them wide open spaces are sometimes found, which are places, where, as in large vallies, some mass of strata has sunk down below the level of their actual bottom. Lastly, the large vallies, as well of Jura as of the Alps, still evidently exhibit that succession of events already described, during which new strata of different species were formed over such other strata, as had subsided in the first catastrophes; after which, at some later period, all these masses of strata, even the very lowest, were again fractured by fresh catastrophes, and new subsidences took place, by which the lowermost strata were brought to sight. These again, no doubt, are generalizations; but they are formed in the field opened to us by the descriptions of M. de Saussure, where such details may be found, as I know to be correct. We have already seen that the island of St. Pierre in the lake of Bienne consists of broken strata of sandstone, while the abrupt sides of the lake are of limestone; this is an example of the last of the above-mentioned operations; and another similar to this will be seen in the valley of Faucigny, by continuing to follow the able observer who has hitherto conducted us, among farther monuments of catastrophes, so strikingly indicative of the cause, by which both the vallies and the cavities of lakes, have been produced.

"Above this village" (of Brezon) "lie rich and "spacious pastures, interspersed with huts, which "are inhabited only during the summer, and are "called *Granges de Solaison.* In one of these I "usually passed the night, when visiting the Brezon, "and the neighbouring mountains. The Granges "de Solaison are commanded to the S. E. by the "*Monts Vergi*, a very high calcareous ridge, the "summits of which I have likewise explored; they "are visible from the environs of Geneva, on the "right of the Mole. This chain runs from N. E. "to S. W. and terminates behind the mountains "which border our road on the right. In the envi-"rons of Siongy, the structure of the last mountain "of the chain may be observed; it is very remark-"able. Its strata, which are horizontal at the sum-"mit, are *bent nearly at right angles*, and descend "thence *perpendicularly* on the N. W. side. They "appear as if they had been incurvated by some vio-"lent power; and in some places they are *separated* "from each other. At the foot of this mountain, "on the south of the road, opens the valley which "leads to the Chartreuse du Reposoir. On ap-"proaching Cluse, we passed under rocks, the thick "strata of which *projected* over the road. These "rocks are contiguous to a mountain, the *pyramidal* "top of which rises to a great height."

168. *a.* In § 106, I quoted the following remark, made by Mr. Playfair, (p. 105.) when speaking of defiles among mountains. "If he," (an observer) "ventures to reason concerning the *cause* of so won-

 "derful

"derful a change, he ascribes it to some great *con-* "*vulsion* of nature, which has *torn* the mountain "asunder.....It is only the *philosopher*, who has deep- "ly meditated on the effects which *action long con-* "*tinued* is able to produce,....who sees in this no- "thing but the gradual working of a *stream*, that "one flowed as high as the *top of the ridge*....." I shall now observe that "it is only a philosopher," such as I am disposed to consider Mr. Playfair, who, having relinquished his first opinion in consequence of such evident phenomena, will have sufficient courage to confess his error: nor can he find any difficulty in convincing himself of the reality of these phenomena; since we shall hereafter see him acknowledge their true *cause*.

"I come now," proceeds M. de Saussure, "to "the mountains, which, on our left, border the val- "ley from Bonne-ville to Cluse. The *Mole* is the "most conspicuous of them: we are surprised to "observe this mountain, which, seen from Geneva, "has the form of a sugar-loaf, stretching itself in "the direction of the valley of the Arve. When "viewed from hence, we perceive it to be crowned "with several summits, which, being all on the same "line, appear from Geneva to form only one. There "is a great *chasm* between two of these summits, "evidently occasioned by the *falling in* of the *in-* "*termediate* part; and indeed the debris are found "accumulated at the foot of the mountain; they "have formed there a very elevated hill, which is at "present covered with vines. The Mole terminates

at

" the junction of the *Giffre* with the Arve; its last " strata *descend rapidly* into the bed of that small " river.

" The mountains which follow the Mole, and " which, in continuation of it, form the southern " side of the valley of the Arve, are low, and little " interesting to the geologist. One of them, how- " ever, is remarkable on account of its *pyramidal* " figure, and of its strata, which *converge* at the " summit, giving it the form of an angular roof..... " Thus the valley stretching between Bonne-ville and " Cluse is hemmed in on both sides by mountains, " which are indeed all calcareous, but their forms " are very various and very irregular, and their strata " seldom horizontal. The flat bottom of the valley " consists of sand, gravel, and rounded pebbles; " and even the *hills which rise on this bottom* are " composed of sandstone."

169. This is an instance of what I have mentioned above, concerning those masses of strata which rise above the levelled bottom of great vallies; they point out *successive revolutions*, in the intervals of which new kinds of strata were formed at the bottom of the sea. It is only, then, by a careful observation of all the phenomena of these great cavities, that "*visible*" traces of their *cause* can be discovered in them; but traces are equally visible in the singular association of chains of mountains and their vallies. We have already seen, in the preceding descriptions, the extraordinary form and situation of

the

the Monts Vergi, considered with respect to the direction of the valley of the Arve; other parts of M. de Saussure's work might supply many more instances of the same kind; but I shall confine myself to what he says, § 284, of the *Vallée du Reposoir*, the entrance of which into that of the Arve we have seen him indicate, at the foot of the last eminence of the Monts Vergi.

"Behind the Monts Vergi," he says, "there is "another valley, which is not visible from the Mole, "although it is of some width. In this valley is si-"tuated the *Chartreuse du Reposoir*.....Beyond it "rise very lofty mountains, which are still calcare-"ous, and which turn their *steep side* towards the "central chain of the Alps. The *Vallée du Repo-"soir* then separates the chains belonging to the in-"terior of the Alps from those which belong to the "external ridges.....Above the convent, on the side "next the interior of the Alps, a *calcareous summit*, "of a very great *height*, and quite *inaccessible*, "strikes the eye; it rises in a thin sheet, like a crest, "above the point of a rock, itself of considerable "elevation. This crest is perforated near its western "edge. The aperture may be discerned from the "convent with a telescope, or even without one, by "those whose sight is strong. This summit is dis-"tinctly seen from the top of the Mole, and even "from our plains: it is likewise visible from the in-"terior of the Alps, to the N. W. above *Salanche*. "The chain, of which it forms a part, becomes gra-"dually lower as it tends towards the valley of the

" Arve, and terminates above the town of Cluse, as
" may likewise be seen from the top of the Mole."

170. When these characters of the chains of mountains and their vallies shall have been more noticed, in those regions, which, for some time past, have been so much frequented, they will show, more forcibly than any other instance, how far the judgment may be misled by prejudices. It would be impossible to assign any other cause to so many theories of the earth, in which it is still maintained that the vallies have been scooped out by the running waters. Such theories have rendered it very necessary to collect within a small compass some of the descriptions dispersed in M. de Saussure's travels, in order to induce the future visiters of those mountains to fix their attention on these important objects. I shall however conclude this selection of facts, by the description of the lake of *Bourget*, the last of those which I had announced, and I shall then resume the great subject of lakes, with a particular view to the explanations which Mr. Playfair has endeavoured to give of them. The following are the words of M. de Saussure, § 1170.

" An excursion to the lake of *Bourget* is a fa-
" vourite amusement with those who drink the wa-
" ters at *Aix*, (in Savoy.) Visiters often cross this
" lake in boats, for the purpose of seeing the *Abbaye*
" *d'Haute-combe*, and an intermittent fountain, which
" issues from a rock near the Abbey, in a very in-
" teresting spot. In the month of October, 1784,

"I observed the temperature of the bottom of the "lake. Its deepest part, I am told, is at the foot "of a rock, which *dips very rapidly* into the lake, "below the castle of *Bourdeaux*. This castle stands "nearly opposite to *Aix*, on the other side of the "water. At this place I let down my thermometer, "at about 200 paces from the bank, and it reached "a depth of 240 feet......This lake is scarcely two "leagues in length, and is from half to three quar- "ters of a league in breadth. It communicates "with the Rhone by a canal, which, according to "the relative height of the waters of the lake and "of the river, sometimes discharges the waters of "the Rhone into the lake, and sometimes those of "the lake into the Rhone.....Being desirous to ascer- "tain with precision the depression of this lake be- "low that of Geneva, I made, in the month of "August, 1790, forty-three observations with the "barometer at Aix, while M. Sennebier made cor- "responding observations at Geneva. I have taken "the mean of these, from which it appears.....that "this lake is 76 toises below the level of the lake "of Geneva.....While my thermometer was in the "water, I inspected the rocks which border the lake "to the westward; they consist of a compact calca- "reous stone, or coarse marble, in which are found "though rarely, fossile shells, such as *cornua ammo- "nis.* Their strata are *dipping* towards the lake "the rock, opposite to which I placed my thermo- "meter, makes an angle of from 50 to 60 degrees "with the horizontal plane: another great rock more "to the north, called *Grateloup*, has an inclination

"o

"of 40 degrees. It is very probable that these
"rocks were not formed in so *inclined* a position.
"I am induced to believe that their base has *sunk*,
"and that this *depression* has produced the *basin*
"which is now occupied by the lake of Bourget."
M. de Saussure here makes this remark, with a view to retract the opinion which he had expressed in his first volume, that strata strongly *inclined* might have been formed in that situation.

171. I could add to the above many other descriptions given by the same excellent observer of the mountainous tracts surrounding the lakes, and of the connexion subsisting between the latter and the phenomena of vallies; from all which useful inferences might be drawn; but I shall confine my extracts to those precise and striking details, which sufficiently evince, against Mr. Playfair's opinion, that the lake of Geneva is not an "*exception*" to other lakes, in respect of the chronometrical phenomenon of the alluvial land formed by the Rhone at its entrance into it: that the *cause* of the production of its cavity is not *local*, as he has imagined, but is the same to which that of the basins of all other lakes, and of all vallies, is to be assigned: and that this is not, as he thinks, a "case," where, in consequence of the deficiency of "*visible causes*," we are "warranted by the rules of philosophical "investigation" to form an "hypothesis," for the sole purpose of explaining it. For the effects of the "*cause*" which has produced the lake of Geneva, together with all similar phenomena, are plainly "visible;"

"visible;" and in order to be convinced of this, it is not even necessary to have recourse to an actual observation of the phenomena themselves, since we are in a manner transported to the spots by the accurate descriptions of M. de Saussure. I would even content myself with referring Mr. Playfair to the *vignette* in the title-page of the third volume of the *Voyages dans les Alpes*, which is that whence I have taken the above descriptions, where he would see a part of the lake of *Mont Cenis*, with the *vertical strata*, which surround it, *dipping* in its waters; and likewise the rocks, which tower above it, and which I have also observed. When spots like these, so frequently met with in the upper parts of high mountains, are attentively examined, it is impossible not to be convinced that *basins of lakes, as well as vallies, are necessary effects of the catastrophes which the strata have undergone.*

172. Returning now to Mr. Playfair, the subject, to which his subsequent observations will introduce us, must excite some surprise; for we shall find him there himself describing the *real cause* of the existence of vallies and the basins of lakes, absolutely unconnected as it is with his hypothesis of the waste occasioned by running waters, and of a dissolution of saline strata. It should seem then that I might have contented myself with the *argumentum ad hominem*, which he will thus afford me; but besides that, not considering the present examination in the light of a controversy, I lay no stress whatever upon such arguments, the generalizations, given by Mr. Playfair,

Playfair, of the phenomena relating to this new subject, do not fix them with the same degree of certainty and precision, which result from M. de Saussure's descriptions, united with the circumstances, which, from my observations, I have had occasion to add. We are going to enter here upon a new field, both of natural philosophy and of natural history, which the discussion of the opinions of Mr. Playfair and Dr. Hutton will render of great importance to Geology in general. The passage of Mr. Playfair's work, immediately following that which has led me into so many details, a passage no less requiring our attention, begins thus, in § 327.

"Another circumstance may also influence the "generation and preservation of lakes; but it is also "one with which we are but *little acquainted.* The "strata, and indeed the whole mineral substances "which forms the basis of our land, have been "*raised up* from the bottom of the sea, by a pro-"gress that should seem in general to have been "gradual and slow. Appearances, however, are "not wanting, which shew, that this progress is not "uniform; and that both *rising* and *sinking* in the "surface of the land, or in the rocks which are the "base of it, have happened within a period of time, "which is by no means of great extent. In this "progress, the elevations and depressions may not "be the same for every spot. They may be partial, "and one part of a stratum, or body of strata, may "*rise* to a greater height, or be more *depressed*, than "another. It is not impossible, that this process

"may

" may affect the *depth* of *lakes*, and change the re
" lative level of their sides and bottom."

173. This passage will suffice for the introductio of the subject: what follows is the developement c the *hypothesis* that partial *depressions* have concurre with a gradual *elevation* of the land in the produc tion of our continents; an hypothesis whereby Mi Playfair endeavours to show a cause of *uncertainty* with regard to the period of the formation of lake Here then we have one *attempt* after another to fu nish an explanation, which ought to have been ev dently established, before the opinion of the excav tion of vallies by running waters had been mai tained: and hence alone, independently on the fac whicn I have adduced above, it might appear hc chimerical this theory must necessarily be. Wi respect to these alternate elevations and depressio of the land, it will be seen hereafter, when I sh come to the objection which I had opposed to t theory of elevation, in my *Lettres sur l'Histoire la Terre et de l'Homme*, that they serve only increase the force of that objection; I shall n therefore enlarge upon the subject in this place. shall also abstain from dwelling upon that which had fully shown in my first letter to Dr. Hutto viz. thnt this operation is laid the more open to tl same objection, by his fundamental theory of tl *elevation of successive continents*, on the same par of the globe. But these accessory points are co nected with the general and most simple hypothes of elevation, which is still maintained by other ge

logis

logists, viz. that the existing continents have been "raised up" from the bottom of the sea. I shall therefore confine myself to this leading point, in the fate of which all its accessories will be involved. For this purpose, I shall go back to § 42 of Mr. Playfair's work, where, after having described those phenomena, respecting which we both agree, in the state of our mineral strata, which, though they all were originally horizontal and continuous, we now find often incurvated, and most generally fractured, with different degrees of inclination, and even in some cases quite vertical, he thus proceeds:

"The highly inclined position, and the manifold "inflexions of the strata, are not the only proofs of "the disturbance that they have suffered, and of the "*violence* with which they have been *forced up* from "their original place. Those *interruptions of their* "*continuity* which are observed, both at the surface "and under it, are evidences of the same fact. It "is plain, that if they remained now in the situation "in which they were at first deposited, they would "never appear to be *suddenly broken off*. No stra-"tum would terminate *abruptly;* but, however its "nature and properties might change, it would con-"stitute an entire and *continued* rock, at least where "the effects of waste and detritus had not produced "a separation. This, however, is very far from be-"ing the actual condition of stratified bodies. Those "that are much inclined, or that make considerable "angles with the horizontal plane, must terminate "abruptly where they come up to the surface. Their

doing

"doing so is a necessary consequence of their position, and furnishes no argument, it may be said, "for their having been disturbed, different from that "which has been already deduced from their inclination. There are, however, instances of a *breach* "*of continuity* in the strata, under the surface, that afford a proof of the *violence* with which they have been "displaced, different from any hitherto mentioned. "Of this nature are the *slips* or *shifts*, that so often "perplex the miner in his subterraneous journey, "and which change at once all those lines and bearings that had hitherto directed his course. When "his mine reaches a certain plane, which is sometimes *perpendicular*, sometimes *oblique* to the horizon, he finds the beds of rock *broken asunder*, "those on the one side of the plane having changed "their place, by sliding in a particular direction along "the face of the others. In this motion they have "sometimes preserved their *parallelism*, that is, the "strata on one side of the slip continue parallel to "those on the other; in other cases, the strata on "each side become *inclined* to one another, though "their identity is still to be recognised by their "possessing the same thickness, and the same "internal characters. These *shifts* are often of "great extent, and must be measured by the quantity of the rock moved, taken in conjunction with "the distance to which it has been carried. In "some instances, a vein is formed at the plane of "the *shift* or *slip*, filled with materials of the kinds "which will be hereafter mentioned; in other instances, the opposite sides of the rock remain con-

"tiguous

"tiguous, or have the interval between them filled
"with soft and unconsolidated earth. All these are
"the undeniable effects of *some great convulsion*,
"which has shaken the very foundations of the earth.
".....The production of the appearances now de-
"scribed, belongs, without doubt, to *different pe-
"riods of time*; and, where slips intersect one another,
"we can often distinguish the less from the more
"*ancient*. They are all, however, of a date *poste-
"rior* to that at which the *waving* and *undulated*
"forms of the strata were acquired, as they do not
"carry with them any marks of the softness of the
"rock, but many of its complete induration.....
"Though such marks of *violence* as have been now
"enumerated are common in some degree to all the
"strata, they abound most among the *primary*, and
"point out these as the part of our globe which has
"been exposed to the greatest vicissitudes. At their
"junction with the *secondary*, or where they emerge,
"as it were, from under the latter, phenomena occur,
"which mark some of those vicissitudes with asto-
"nishing precision.....Where the *primary schistus*
"rises in beds almost *vertical*, it is" (sometimes)
"covered by horizontal layers of *secondary sand-
"stone*," (or calcareous stone,) "which last are pe-
"netrated by the irregular tops of the schistus, and
"also involve *fragments* of that rock, some *angular*,
"others *round* and *smooth*, as if worn by attrition."

174. To the accuracy of these descriptions of phenomena, and to the general conclusion drawn from them, viz. that they can be ascribed only to "*some great con-
"vulsions*,"

"*vulsions,*" I give a perfect assent. But what was the nature of those "convulsions?" And what motions of the strata did they produce? It is indeed evident that the relative level of the fractured masses of strata has been changed; but were these revolutions effected by the *elevation* of the *highest* of the masses, or by the *subsidence* of the *lowest?* Such is the question at which we must ultimately arrive. I shall not, therefore, dwell upon the passages immediately following, in which Mr. Playfair attempts to explain, by the theory of elevations succeeded by depressions, the last phenomenon which he had mentioned; namely, that of new strata formed on the ruins of the former; for this again is only an accessory circumstance, the application of which depends on the principal question. I shall therefore pass on to §45, where he states his reasons in favour of the theory of elevation, as opposed to that of subsidence.

"On the whole, therefore," he says, "by comparing the actual position of the strata, their erectness, their curvature, the interruptions of their continuity, and the transverse stratification of the secondary in respect of the primary, with the regular and level situation which the same strata must have originally possessed, we have a complete demonstration of their having been *disturbed, torn asunder,* and *moved angularly,* by a *force* that has, in general, been directed *from below upwards.* In establishing this conclusion, we have reasoned more from the facts which relate to the *angular elevation* of the strata, than from those "which

"which relate to their *absolute elevation*, or their "translation to a greater distance from the centre "of the earth. This has been done, because the "appearances, which respect the absolute *lifting* "*up* of the strata are more ambiguous than those "which respect the change of their *angular* position. "The former might be accounted for, could they be "separated from the latter, in *two* ways, viz. either "by the retreat of the sea, or the raising up of the "land; but the latter can be explained only in *one* "way, and *force us of necessity* to acknowledge the "existence of an *expanding power*, which has acted "on the strata with incredible energy, and has been "directed from the *centre* towards the *circumfe-* "*rence*."

174. *a*. This proves only that Mr. Playfair has paid no attention whatever to my first letter to Dr. Hutton, in which this subject was treated. For, in the first place, the angular motion of the strata, that is to say, the whirling of the ruptured and separated masses, by which the strata, originally horizontal, became inclined, and even vertical, is no doubt a fair criterion of the respective merits of the two theories; but we shall find that it will prove fatal to that of elevation. With respect to the other theory, which is mine, it is evident that Mr. Playfair is unacquainted with it. For, though I ascribe the existence of our present continents to the subsidence, not of the bed of the ocean, but of former continents, the place of which the sea now occupies at a lower level, yet I do not ascribe to this revolution

those catastrophes of our strata, of which we now observe the effects. And here again, Dr. Hutton's theory, referring all these phenomena to the act of elevation, its advocates are reduced to the necessity of having recourse, first to *elevation*, in order to account for the rupture and displacing of the *primitive strata*, and then to partial *depressions*, for the purpose of again reducing some parts of them under the level of the sea, there to receive the *secondary strata*. I have passed over the passages in which this is expressed by Mr. Playfair, since I do not stand in need of the arguments with which they would have furnished me against the doctrine of elevation.

175. Secondary strata, incumbent, not only on primitive, but also on such antecedent secondary strata, as had already undergone their first catastrophes, and contained the remains of marine animals, were surely formed in the sea, as Mr. Playfair likewise maintains. These catastrophes, however, were much less considerable than those which took place after the formation of the latest of the secondary strata, consisting of a certain species of sandstone. I have pointed out above some monuments of these effects in M. de Saussure's descriptions; and others, still more characteristic, of which it is not yet time to speak, concur in proving that all these catastrophes happened, while our strata constituted the bed of the ocean. It was there that those subsidences took place, by which the strata were fractured, and the angular movements produced, which, in so many

cases,

cases, placed them upright upon their edges; and there the masses remaining at a higher level had room afforded them to turn in the empty spaces, left by those which had sunk lower: the necessity of which spaces to such movements will evidently appear to be absolutely subversive of the whole theory of elevation. But here I shall first remark, in conformity to what I have before announced, that, in the above description, Mr. Playfair has himself indicated a *cause*, adequate to the existence of vallies, and of the basins of lakes, and correspondent with all their phenomena, without any want of those other causes, to which he has had recourse. The angular movements, pointed out by him, are real; but he has not considered what those masses are, by which they have been undergone; they are the very masses which form *chains of mountains;* as may be seen in M. de Saussure's descriptions. Now how could such masses have possibly suffered these angular movements, without leaving between them the intervals called *vallies*, while those parts of them which were still more depressed became the *basins* of lakes? Hence it is, that, on examining the bottoms of vallies, we everywhere find, that, far from having been *hollowed out* by running waters, they have, on the contrary, been *raised* by the debris brought down by streams; and that there, as well as at the entrance of rivers into lakes, we perceive large fragments of strata, rising above the rubbish which the waters have spread over the surface. This origin of the vallies equally belongs to both the rival theories, that of the elevation of the highest parts, and that of the

 depression

depression of the lowest; for both will equally well explain the fact, that it is by the "*angular motions*" of masses, capable of forming *chains of mountains*, that *vallies* have been left between them. This is not then, as Mr. Playfair supposes it, an immediate criterion of these theories; but it will, nevertheless, afford one, as will be seen in the examination, to which I shall now proceed, of the theory of elevation, considered in itself.

176. All the geologists, who have adopted this theory, have had recourse to the agency of *heat;* supposing its action, however, to have been analogous to *volcanic* operations, and to have been exerted only once, for the purpose of forcing up our continents above the level of the sea: whereas, according to Dr. Hutton's theory, the globe has a constant *internal heat*; and to this heat he ascribes, in the first place, the *consolidation of the mineral strata*, produced, as he supposes, from the *detritus* of preceding continents. This hypothesis, therefore, will lead us to some interesting discussions, on subjects of both natural philosophy and natural history, which are introduced by Mr. Playfair in § 46, after what we have seen him adduce as a proof of "the existence of an expansive power, which has acted on the strata with incredible energy, and has been directed from the centre toward the circumference."

"When we are *assured*," says he, "of the *existence of such a power as this in the mineral regions*, we should argue with singular inconsistency if we did

"did not ascribe to it all the other appearances of "motion in those regions, which it is adequate to "produce. If nature in her subterraneous abodes "is provided with a force that could *burst asunder* "the massy pavement of the globe, and place the "*fragments upright upon their edges*, could she not, "by the same effort, *raise* them from the greatest "*depths of the sea*, to the highest *elevation of the* "*land?* The *cause* that is adequate to one of these "effects, is adequate to them both together; for it "is a principle well known in mechanical philoso-"phy, that the force which produces a parallel mo-"tion, may, according to the way in which it is "applied, produce also an angular motion, without "any diminution of the former effect. It would, "therefore, be extremely unphilosophical to suppose, "that any other *cause* has changed the relative level "of the strata, and the surface of the sea, than "that which has, in so many cases, *raised* the strata "from a *horizontal* to a *highly inclined*, or even *ver-*"*tical* situation: it would be to introduce the action "of more causes than the phenomena require, and "to forget, that nature, whose operations we are "endeavouring to trace, combines the possession of "infinite resources with the most economical appli-"cation of them.

"From all, therefore, that relates to the *position* "of the strata, I think I am justified in affirming, "that their disturbance and removal from the place "of their original formation, by a force directed "*from below upwards*, is a *fact* in the natural his-"tory

"tory of the earth, as perfectly *ascertained* as any "thing which *is not the subject of immediate observation.* As to the *power* by which this great effect "has been produced, we cannot expect to decide "with *equal evidence,* but must be contented to pass "from what is *certain* to what is *probable.* We "may, then, remark, that of the *forces* in nature to "which our experience does in any degree extend, "none seems so capable of the *effect* we would "ascribe to it, as the *expansive power of heat;* a "power to which *no limits can be set,* and one, "which, on grounds quite *independent of the elevation* of the strata, has been already concluded to "act with *great energy* in the subterraneous regions. "We have, indeed, no other *alternative,* but either "to *adopt this explanation,* or to ascribe the *facts* in "question to some *secret and unknown cause,* though "we are ignorant of its nature, and have no *evidence of its existence.*

"We are therefore to suppose, that the *power* of "the same *subterraneous heat,* which *consolidated and mineralized the strata at the bottom of the sea,* has since *raised them up to the height* at "which they are now placed, and has given them "the various *inclinations* to the horizon which they "are found actually to possess.

"The probability of this hypothesis will be greatly "increased, when it is considered, that, besides "those now enumerated, there are other indications "of *movement* among the bodies of the mineral "kingdom,

"kingdom, where *effects* of *heat* more characteristic "than simple *expansion* are clearly to be discovered. "Thus, on examining the marks of disorder and "movement which are found among the strata, it "cannot fail to be observed, that notwithstanding "the fracture and dislocation, of which they afford "so many examples, there are few *empty spaces* to "be met with among them, as far as our observation "extends. The *breaches* and *separations* are nu- "merous, and distinct; but they are, for the most "part, completely *filled up* with *minerals* of a kind "quite different from the rock on each side of them, "and remarkable for containing no *vestiges of stra-* "*tification*...We are thus led to consider the *unstra-* "*tified fossils*, the second of the divisions into which "the whole mineral kingdom, viewed geologically, "*ought* to be distinguished. These fossils are im- "mediately *connected* with the *disturbance* of the "strata, and appear, in many instances, to have "been the *instruments of their elevation*."

177. Such is Mr. Playfair's statement of the Huttonian theory: every thing in it is presented as susceptible of *rigid demonstration;* and this circumstance will render its discussion highly interesting, on account of the several objects to which it will lead us, when these objects shall have been fixed, by an analysis of the above exposition. It may already be understood why Mr. Playfair omits all mention of my objections against that theory; those who suppose, as he does, that the propositions of the theory maintained are supported by *direct proofs,*

must

must necessarily consider all *objections* against it as frivolous, and not worth the trouble of discussion. A similar motive might actuate me also; but it shall not withhold me from a due examination of the foregoing passages.

178. Every thing in the above statement depends on *two propositions*, which Mr. Playfair considers as separately demonstrated, and which, concurring in the same point, must thus serve to establish it on the most solid foundation: one is, that our *continents have been raised up from the bottom of the sea;* the other, that the *mineral strata*, of which they are composed, *have been consolidated by a great heat.* The pretended *demonstration*, given by Mr. Playfair of the former, is as follows. "The appearances "which respect the change of the *angular position* "of the strata can be explained *only in one way*, "and force us of necessity to acknowledge the ex-"istence of an *expanding power*, which has acted on "them with incredible energy, and has been directed "*from the centre toward the circumference.*" There is, however, another "*way*" in which they may be explained, though it is here so peremptorily excluded; and that is the *subsidence* of the lowest parts. Mr. Playfair would have been justified in rejecting it, thus as of course, if it supposed the angular movements observed in the strata to have accompanied that sinking, which gave birth to our continents; but I have shown above that such a representation of my theory was erroneous, and proceeded solely from his not having considered what I had said on this subject in my first letter

ter to Dr. Hutton. Now, with regard to an "*effect*" which has been explained in "*two*" different "*ways*," especially when it is acknowledged "*not* to be" *the* "*subject of immediate observation*," it surely is not allowable to decide on the *certainty* of *one* of those "ways," until we have given an exposition of the other, and even discussed the arguments adduced in its favour by those who have adopted it. If Mr. Playfair had done this, which certainly was but reasonable, he would have become aware of the error, into which he had fallen, as I have remarked above, with respect to my system, and would have been obliged to look for some other *demonstration* of his own theory.

179. I had also explained in what manner the subsidences of the strata had been produced, together with all the ensuing consequences, as manifested by the phenomena; the whole of which formed a complete system: while, with respect to his own hypothesis, Mr. Playfair says only: "As to the "*power* by which this great effect has been produced, "we cannot expect to decide with *equal evidence*." Now this is saying very little, considering that the whole theory rests on that point. However, from this expression, it is natural to expect that he will at least establish the *probability* both of the *existence* of that "power," (an *intense heat*, as he supposes,) and of a certain *mode of its action*. It is therefore with a view to that point that I shall proceed to examine what he says on these objects. With regard to the first, namely, the *existence* of the power supposed,

posed, he does not ground it upon any phenomenon connected with the act of elevation; he *infers* it from one, which bears no kind of relation to it, namely, the *solidity* of the greatest part of the mineral strata, the consolidation of which he *supposes* to have been produced by an *intense heat*. Thus, when speaking of this power, as having "burst "asunder and raised up the massy pavement of the "globe," he calls it, "a power, which, on grounds "quite *independent* of the elevation of the strata, "has been already concluded to act with *great ener-"gy* in the subterraneous regions." This constitutes the whole of his *demonstration*, which I shall consider in due order; when it will be seen to rest only on that hypothesis respecting the *origin* of our strata, which deduces them *from the detritus of other continents*.

180. The following, therefore, is the progress of these pretended demonstrations.—Our *mineral strata* were produced from the *materials* of ancient continents previously destroyed.—But such materials could be *consolidated* only by *fusion*, at a degree of *heat* capable of producing that effect.—Therefore, an excessive heat *exists* in the interior parts of the globe. Such is the kind of *demonstration*, whence Mr. Playfair deduces the following inference: "When "we are *assured* of the existence of *such a power*..... "we should argue with singular inconsistency if we "did not ascribe to it *all the other appearances* of "motion in those regions, which it is adequate to "produce.....We are therefore to *suppose* that the "power

"power of the same *subterraneous heat*, which has "*consolidated* and *mineralized the strata* at the "bottom of the sea, has since *raised them up*." It is therefore evident that the whole of this demonstration is grounded on the *supposition*, that our mineral strata were produced from the *materials of continents which had been destroyed;* and this having an immediate reference to the *origin* of our strata, a most important object in geology, I shall presently examine the above explanation of their existence, comparing it with that, which these geologists reject.

181. The continuation, however, of this analysis, will offer us a previous object of thorough examination. The "*power*" here supposed of *fusion* is still not a power of *elevation;* the intermediate idea of *expansion* is wanting. On this subject Mr. Playfair contents himself with saying, that "the *expansive* "*power of heat* is a *power* to which *no limits can be* "*set*." Now this is saying nothing, unless the *mode* be explained in which this power is exerted; but Mr. Playfair leaves that for others to investigate; to which investigation I therefore must proceed. It cannot be by the *dilatation of solids* or *liquids;* for, with respect to them, the effects of heat are confined to very narrow "*limits;*" it *liquefies* solids, and reduces liquids to *vapour*. Now, when heat produces this last effect, or, in general, when any *expansible fluid* results from it, its "*power*" may indeed be considered as not having any assignable "*limit;*" but it is the *fluid* itself, not *heat* immediately, which exercises that

that power, in proportion to its *density*, the increase of which may be really looked upon as *unlimited.* This therefore may be, and probably is, the *sense* which Mr. Playfair affixes to his words; but they do not express it; and perhaps he has purposely kept it out of sight, because it is on this only really possible *sense* that my objection is grounded. It could be only, I repeat, by an extreme *density* of some *expansible fluid*, that such a mass as that of our continents could have been *raised* from the *bottom of the sea*; but the moment that this power had *fractured* the mass, the greatest part of the fluid would have made its *escape* through the *openings*; and its power being thus spent, the fractured mass would necessarily have fallen down again in ruins to the depths of the ocean. This would have been the inevitable consequence of the raising up of our continents, by such a *power*; and as a peremptory refutation of the theory, it remains for me to show, that, though Mr. Playfair avoids expressing it, such was, however, Dr. Hutton's conception of the effect of *heat:* this I shall do by quoting his own expressions, after having made another previous remark.

182. Although Mr. Playfair makes no express mention of my objection to Dr. Hutton, he seems to have it in view in the following passage of his statement: " The *breaches* and *separations* are numerous " and distinct; but they are, *for the most part*, " completely *filled up* with *minerals* of a kind quite " different from the rock on each side of them, and " remarkable for containing no *vestiges of stratifica-* " *tion*,

" *tion.* We are thus led to consider the *unstratified* " *fossils*, the second of the divisions into which the " whole *mineral* kingdom, viewed geologically, *ought* " to be distinguished." Two distinct facts are here taken for granted, viz. that *the breaches and separations in the strata are for the most part completely filled up;*—and that there is a great class of *minerals, containing no vestiges of stratification.* Both these objects are very important in geology, and require to be distinctly discussed.

183. Thus a close analysis of the exposition given by Mr. Playfair of the theory, which considers *elevation* as the cause of the existence of our continents, has been productive of several geological questions, all of them entitled to the greatest attention from those, who wish to know what may really be expected from the science to which they belong. It may be observed first, that these questions, though distinct from each other, are mutually connected together; and further, that they relate also to other objects, of which I have allowed myself only a simple mention; as they could not have been discussed any otherwise than in reference to this connexion, which, when duly treated, will prove sufficiently decisive with respect to each of them. The first of the stated questions which I propose examining, as being the least dependent on others, and because the consequence, which will result from its decision, will lead to an analogous object of greater importance in the following discussions, is this: Are " *the breaches* " *and separations*" in the strata " for the most part " completely

"completely *filled up*," as maintained by Mr. Playfair?

184. And here he appears to confine his attention to the *metallic veins;* but it is in a very vague manner. We agree respecting their *origin*, in regard which I have also the satisfaction of finding my opinion coincide with that of Mr. WERNER; they are *fissures* produced in the strata, which have been filled up with *extraneous substances*, called the *gangue* of the veins. But Mr. Playfair and I begin to differ on a point which respects the nature of this gangue, or content of the fissure; it is the first species of the class of substances by him asserted to "*contain no vestiges of stratification.*" Now, though the stratification here is not similar to that of the strata themselves, it is, nevertheless, very distinctly marked, and ought to be known by those, who attempt, as Mr. Playfair does, to ascertain how the clefts were filled up. It is a *stratification*, which, for the most part, has extended symmetrically from *either side*, and united in the *middle;* where, the incrustations being of an irregular thickness, empty spaces were necessarily left between them when they came to meet: it is there that the finest crystallizations are found, those *druses* which form the ornament of cabinets. When I was at *Freyberg*, Mr. Werner gave me a direct proof, as well of the metallic veins having originally been fissures, as of the manner in which they had been filled up by successive *strata*. He showed me a vein, which, after being filled up, in the first instance, with extraneous substances,

substances, had a second time been split asunder through the middle of that gangue, a new one being subsequently formed in the cleft, the strata of which corresponded also on either side. For this Mr. Playfair cannot account by the mode in which he supposes those veins to have been filled up, viz. by substances which "appear, in many instances, "to have been the instruments of the elevation of "the strata."

185. But in order to understand the nature of metallic veins, it is not sufficient to consider them as having been produced by fissures in the strata; for a circumstance frequently attends them, which has caused this opinion to be rejected by many geologists; amongst others, by M. DE LA METHERIE, and by M. CHARPENTIER, *Conseiller des Mines* at *Freyberg*, and a very skilful mineralogist. Metallic veins are often found highly inclined, and of considerable thickness: there are, indeed, but few, which are not thus characterised; and this circumstance even accounts for the *shifts*, mentioned by Mr. Playfair; for there could be none in veins which were *vertical*. Shifts are occasioned by new fissures in a *vertical* direction, passing through the veins, the sides of which have changed their relative level. Now when a metallic vein of a certain thickness is highly inclined, the geologists of whom I am now speaking, deduce thence an argument unfavourable to the opinion that the veins have been fissures; since, in that case, before they could have been filled up by the gangue, the *roof*, or upper part, having

having no support, would have fallen in upon the *wall*, or inferior part. This objection cannot be answered, unless the whole series of catastrophes, which the strata have undergone, be collectively considered; but, when that is done, it affords a demonstration, that the inference drawn by these geologists from the phenomenon just noticed, viz. that the metallic veins were formed together with the mountains, is erroneous.

186. All the strata, which now compose mountains, were formed originally in an horizontal position; this is attested by the strata of breccia, and by those which contain marine bodies; but it has been shown that they have undergone fractures and angular movements, in consequence of which, the strata of such masses as our mountains are found to be highly inclined, and even vertical. It has likewise been shown that these catastrophes did not take place at one and the same time, since, after the first convulsions, which were inconsiderable, *secondary strata* were formed on the *primitive*, which had been already fractured; and that, at a subsequent period, the whole mass was subjected to more violent catastrophes: now hence we may collect the history of the veins. They were originally wide fissures, formed *vertically* in the mass of the strata, and were filled up with extraneous matter, while in that position; but afterwards these veins participated in all the catastrophes suffered by the strata, being enclosed in the great masses which underwent angular movements; and thence it is that their sections, like those of

of the strata, occur in the sides of some vallies, and on the summits, where, in the language of miners, they *basset* out.

187. It was, then subsequently to the catastrophes which had occasioned vertical fissures in the strata, that the first mineral veins were filled up. The false veins, (or shifts,) produced by some fresh convulsion during the same period, were filled up with other minerals, and were afterwards fractured and displaced. But the *vacuities*, formed during the period of the greater catastrophes which followed, were not *filled up*; and Mr. Playfair is, therefore, mistaken, if he thinks that every passage, capable of affording an escape to the expansible fluid, by which the continents were to be raised up, was thus closed. For at this time numerous *caverns* were formed in the interior parts of the masses of strata, constituting mountains. I have shown, while following M. de Saussure in his descriptions, that many phenomena of streams among mountains prove that such caverns exist; and a great many of them are immediately known to us, such as those in the metalliferous mountains of *Derbyshire*, and of *Hartz*. By filling the mineral veins, therefore, in order to close up one kind of *outlet* through which the expansible fluid might have passed, no purpose could be answered; so many new passages having been opened for it by the catastrophes, which happened subsequently to the formation of those veins.

188. The contents of mineral veins form the first

of which Mr. Playfair treats, upon coming to the class of substances, in which he contends that "no "vestiges of stratification" occur; an assertion here shown to be inaccurate. He then passes on to *basaltes* and *whinstone*, considering them both as substances, which, in a state of *fusion*, were forcibly injected among the strata. This is a point much contested among mineralogists, and one which has divided them into two distinct classes, known by the appellation of *Vulcanists* and *Neptunists*; the former considering these substances as *volcanic*, the latter, as having been produced by the *sea*, like the other strata. I shall not dwell on this difference of opinion, because it is foreign from our subject; for we are speaking of the *fractures* and *angular movements* of the strata: while the basaltes and the whinstone having undergone these catastrophes together with the other strata, cannot be ranked in the number of those substances, which have served as "in"struments" in such catastrophes. But another considerable class of substances, placed by Mr. Playfair in the number of those which exhibit "no "vestiges of stratification," viz. *granite* and *porphyry*, will lead us to a very important question, connected with all those already indicated as resulting from an analysis of Mr. Playfair's arguments, in favour of the theory of elevation; to the examination of this question I shall now proceed.

189. I have shown in § 180, that the Huttonian theory wholly rests on the proposition: "That our "mineral strata were produced from the waste of "*ancient*

" *ancient continents.*" I shall therefore begin with examining this proposition, one of the most singular ever advanced in geology. The strata of our present continents could not be considered as the product of others more "*ancient*," unless they were supposed to be now preparing the *materials of future continents*: this, accordingly, is taken for granted; and it is alleged, in proof of it, that running waters have carried down to the sea all the materials which they have collected in excavating vallies, and even much more, as the whole surface of our continents is supposed to be already considerably *lowered*. But I have proved that, in fact, running waters do not carry on these pretended operations; and that their real effects are tending to their *end*. It would also be necessary for the support of this hypothesis, that the sediments, formed by rivers at their mouths, should be diffused over *the whole bed of the sea*; whereas I have proved, on the contrary, that they remain along the *coasts*. But let us for the present pass over these points, and allow all that has been excavated from the vallies, and much more, to have been propelled by the rivers to the ocean, and there scattered over the whole of its bed.

190. The phenomenon still to be explained consists in the strata, of so many different genera and species, which *are laid one upon another* throughout the whole extent of our continents, and consequently must have been in the same position at the bottom of the ancient sea: and all these are supposed to have been produced by the sediments of

 rivers

rivers belonging to ancient continents. Now whoever has observed the sediments of rivers on the coasts, must know that they are composed of a confused *mixture* of all the minute particles, detached from the different kinds of strata, through which the rivers, and their various branches, have happened to flow. This being a well-known fact, could the most lively fancy invent any *cause*, which, in the sea, should not only *separate* these particles, according to their different genera and species, but retain them thus *separated*, until the destruction was completed of the continents whence they had proceeded, in order afterwards to diffuse them over the bottom of the sea, and there to *arrange* all those several genera and species, in that regular manner, in which the various strata of our continents are everywhere disposed? This consideration, which arraigns the whole theory, I had already presented in my first letter to Dr. Hutton, though neither he, nor Mr. Playfair, have at all adverted to it in their works.

191. Thus do they pass over impossibilities, while they nevertheless consider their theory as *demonstrated*. In the same manner, we have seen Mr. Playfair rest the hypothesis of elevation by heat on the *evidence* of a great *internal heat* of the globe, by affirming that heat to be *demonstrated* by the *induration* of the strata: although, in my first letter to Dr. Hutton, I had opposed to this cause of consolidation two arguments, neither of which he has noticed. The first was deduced from the remains of *marine animals* found in so many solid strata, as well

as

as in loose beds, and in the concretions formed in these last; and, after having mentioned the facts, I thus proceeded: "The sea-shells, mentioned in many "of the above examples, lead me to a more general "objection against your hypothesis. It would be "sufficient to examine these bodies, to be convinced "that the concretions, in which they are found, had "not undergone a *heat of fusion*. But, moreover, "how could you conceive that the animals them-"selves, of which these are the remains, had *lived* "upon these very strata, which you suppose to have "been successively *consolidated by fusion?* Here "ends every appearance of *possibility;* and I might "have confined myself to this single objection, had "I not thought it preferable to follow all the argu-"ments, which you have adduced in favour of your "hypothesis." The second of my arguments was drawn from the intermixture of the *loose* with the *stony* strata, even where both are of the same kind, and, among other instances, I had in view those strata of soft marle, intermixed with others of calcareous stone, which are described in § 160. But indeed this is a very common phenomenon, and is frequently observable in the cliffs of the British coasts. How is it then, that, without an attempt to answer these peremptory objections, nay, even without noticing them at all, Mr. Playfair can say, "We are *assured* "of the existence of such a *power* in the *mineral* "*regions?*

193. But the following passage seems to explain the reason, which determined him in favour of this cause

cause of the consolidation of the strata. "We have "indeed no other *alternative*, but either to adopt "this *explanation*, or to ascribe the facts in question "to some *secret* and *unknown* cause, though we are "ignorant of its nature, and have no *evidence* of its "*existence*." The theory, thus characterised by Mr. Playfair, is that which has been received by the most eminent geologists, among others, by M. de Dolomieu and de Saussure, viz. that our mineral strata are the product of *chemical precipitations* from a *liquid*, which originally encompassed the whole globe. But we have seen that Mr. Playfair is desirous of banishing from geology every idea of an assignable *commencement* to the operations, of which we see the monuments on our globe. To effect this purpose, it would be necessary that he should prove our mineral strata to have been produced from those of continents previously destroyed, which themselves had been formed from the ruins of other continents, without any assignable limit. Now, if we attend to all that has been advanced, both by Mr. Playfair and Dr. Hutton, and particularly by the latter in the first and seventh chapters of his Theory of the Earth, with a view to set aside *chemical operations*, it will be obvious that all their objections are directed to this point, viz. that *chemical operations* cannot explain the groundless hypothesis, of the transformation of the *remains* of ancient continents into *solid strata*, such as ours: and thus the action of *extreme heat* is the only remaining means of accounting for that imaginary transformation. This is indeed the whole argument; for no instance is adduced to prove

that *heat* itself is capable of producing an effect of this nature; and as they suppose such operations to have been carried on only at the *bottom of the sea* they think themselves under no obligation to give any examples of them on the *surface of the earth*. But let us now come to the real system of *chemical precipitations*, which these geologists could not thus have misconceived, if they had sufficiently considered its effects.

194. It is now generally acknowledged, even by Dr. Hutton and Mr. Playfair, who except only *granite*, and the substances of the same class, that all our mineral substances were formed by *deposites* of some kind, at the bottom of the ancient sea; and that not partially, but throughout its entire extent, their *strata* constituting the whole observable mass of our continents. And here an absolute dilemma presents itself. There are but two possible sources of the matters forming these strata at the bottom of the ancient sea: they must either have proceeded from the *lands*, which surrounded it; or they must have been *separated* from the *liquid*, which originally formed it. Now, since dilemmas have been in use, for the purpose of establishing the truth of one proposition, by excluding the only other which could be brought into competition with it, I do not think that any more peremptory and irresistible cause of *exclusion* has ever been brought forward, than that, which I have stated above, against one of the propositions of this dilemma, respecting the source of the substances forming our mineral strata; substances which, most undoubtedly, did

did *not* proceed from other continents. Nor is this argument applicable only to the Huttonian theory, but to many others also, in which, with different modifications, recourse has been had either to the action of rivers and of the sea upon existing continents; for the purpose of furnishing the materials of new continents; or to operations of the sea within its bosom, for that of transforming ancient stratified deposites, or other masses, into new and solid strata.

195. We find then that *chemical precipitations* alone remain, to explain the existence of *strata* such as ours, which, at the same time, are in perfect conformity to the nature of that cause. This might have seemed doubtful, so long as our strata were considered solely in their *present state:* but, since it has been demonstrated by M. de Saussure that they were originally formed in an *horizontal* position, for the most part *parallel* one to another, and disposed according to their several *genera* and *species;* so that their actual state is to be ascribed to *fractures*, changes of relative *level*, and *angular movements* of separate masses: in a word, since their *formation* has been ascertained to be very distinct from their *actual position*; (as is admitted by Dr. Hutton;) the correspondence of the former with successive chemical precipitations, of different genera and species, from one and the same *liquid*, most evidently appears. This is the cause, which was first assigned to them by M. de Saussure, and I have adopted it after him and M. de Dolomieu. Supposing, therefore, that we could proceed no farther in this explanation of the

the origin of our mineral strata, nor determine any thing as to the *manner* of the operation; it would still remain certain, both from the *exclusion* of every other origin, and from the conformity of the effect with the nature of the cause, that they were produced by *chemical precipitations in the liquid of the ancient sea.*

196. But we are not confined within such narrow limits. We cannot, it is true, apply the *particular laws* of chemistry to the formation of our strata, for reasons which I shall presently explain; but its *general laws* throw great light on these operations. M. de Dolomieu, with whom M. de Saussure and I coincided in opinion, expressed long ago his surprise, that some mineralogists should have applied themselves to the search of a *menstruum*, or even of several *menstrua*, capable of holding the substances of our strata together in solution. Such a search has hitherto proved vain; for we cannot go back, by the *specific affinities* discovered in our experiments, to that state of things, which existed on our globe once only, and which then soon began to undergo successive changes, by means of the precipitations themselves. At that period, the *primordial liquid,* from the reciprocal affinities of all its ingredients with each other and with *water,* contained all the substances, which, at present, are divided into four general masses, viz. our *strata,* the *atmosphere,* the *water of the sea,* and common *water.* How is it possible to recompose a *liquid,* formed of all these substances, while the *elementary* ingredients, whence the

the affinities then arose, are now dispersed in certain *combinations*, of which we have but a very imperfect knowledge?

197. Let us now come to a more general object. What is a *menstruum?* The only answer to this question, with which we are furnished by the united results of all our experiments, is, that a menstruum is *water*, already combined by affinity with some ingredients, in such a manner, that the *compound* is capable, in consequence of new affinities arising in it, of uniting itself with other substances. M. BERTHOLLET, who has been more successful than any other chemist in the observation of those affinities which result from a combination of other affinities, has already rendered many of them manifest by actual experiment. A new ingredient, united by affinity to a menstruum, gives birth to new affinities: this is a *general law*, to which we can assign no limits; although, in the combinations which we are able to produce, it is confined within very narrow bounds; because we are unacquainted with a multitude of the ingredients, which enter into the composition of the different substances of our globe. Of this we have sufficient proof, from the impossibility of *recomposing* the mineral substances with the coercible ingredients furnished by our analyses. For we are even ignorant, as M. de Dolomieu observes, in what *order* the ingredients, which we obtain, were combined; whether it was in the first molecules, or in the aggregation of these molecules into different solid bodies, manifesting nearly the same ingredients

in

n our analyses, though exhibiting very different properties. This is, however, though expressed in so general a manner, an essential point of consideration, on account of the succession of affinities known to arise from first combinations.

198. We are then authorised to say—that the *primordial liquid*, in consequence of a multitude of combined affinities, contained the greatest part of the ingredients, which at present compose the known substances of our globe;—and that they have been separated from it by successive *precipitations*, produced, both by new ingredients ascending from the interior parts of the globe, and by the *separation* of other ingredients, which, being disengaged in the form of *expansible fluids*, have successively composed our *atmosphere*. Now, the knowledge of the astonishing composition of the atmosphere, that essential part of the substances of our globe, a knowledge indispensable in considering the operations which have taken place upon it, is obtained, only in proportion to our care in combining *meteorological* chemistry with that of our laboratories. It is not difficult to determine (as I have shown from the connexion with other phenomena) how new ingredients may have ascended from the *interior* of the globe; and geology, at the same time, by very distinct monuments in our mineral strata, furnishes us with the knowledge of the effects, which were thence produced in organised beings, marine as well as terrestrial, on account of the corresponding changes of the liquid of the sea, and of the atmosphere. But for

for the details of this theory I must here refer to my *Lettres au Professor Blumenbach*, Paris edition.

199. The several discussions entered upon above, although introduced by Dr. Hutton's theory, (the hypotheses of which, as Mr. Playfair acknowledges, have not excited much attention) serve nevertheless to throw considerable light on the geological points to which they relate. This will also be the case in regard to the following points, the first of which is the theory of *elevation* considered in itself; a theory which has made part of some other systems of geology. We have seen that Mr. Playfair contents himself, relatively to this great operation, with saying, that the "*expansive power* of *heat* has *no* "*limits*;" without pointing out the specific mode of its action. This renders it necessary to go back to the exposition, which Dr. Hutton himself has given of the grounds of this hypothesis; but it will lead us to several important remarks. The subject is introduced at page 141 of his first volume.

"A stream of melted lava flows from the sides of "Mount Ætna. Here is a column of weighty mat-"ter raised from a great depth below, to an immense "height above, the level of the sea; and *rocks* of "an *enormous size* are projected from its orifice some "miles into the air. Every one acknowleges that "there is the *liquefying* power and *expansive force* "of subterranean *fire*, or violent *heat*. But, that "*Sicily* itself had been raised from the bottom of "the ocean, and that the *marble* called *Sicilian jas-*

per,

" *per*, had its solidity upon the same principle with " *lava*, would stumble many a naturalist to acknow- " ledge. Nevertheless, I have in my possession a " table of this marble, from which it is demonstrated, " that this *calcareous stone* had *flowed*, and been in " such a state of *fusion* and *fluidity* as *lava*.

" Here is a comparison formed of two mineral " substances, to which it is of the highest importance " to attend. The *solidity* and present state of this " is commonly thought to be the operation of *fire*; " of the other again, it is thought to be that of *wa-* " *ter*. This, however, is not the case. The imme- " diate state and condition of both these bodies are " now to be considered as equally the effect of *fire*, " or *heat*. The reason of our forming such a different " judgment with regard to these two subjects is this; " we see, in the one case, the more immediate connec- " tion of the cause and the effect, while, in the other, " we have only the effects from whence we are in " science to investigate the cause. But, if it were " necessary always to see this immediate connection, " in order to acknowledge the operation of a *power*, " which, at present, is extinguished in the *effect*, we " should lose the benefit of science, or general prin- " ciples whence particulars may be deduced, and we " should be able to reason no better than the brute. " Man is made for science; he reasons from effects " to causes, and from causes to effects; but he does " not always reason without error. In reasoning, " therefore, from appearances which are particular, " care must be taken how we generalize; we should " be

" be cautious not to attribute to *nature*, laws which
" may perhaps be only of our own *invention*.

" The immediate question now before us is not, if
" the *subterraneous fire*, or *elevating power*, which
" we perceive sometimes as operating with such en-
" ergy, be the *consolidating* cause of strata formed
" at the bottom of the sea; nor, if that *power* be the
" means of making *land* appear above the *surface of*
" *water*: for, though this be the end we want to ar-
" rive at ultimately, the question at present in agi-
" tation, respects the *laws of nature, or the gene-*
" *rality of particular appearances*.

" Has the globe within it such an active power, as
" fits it for the renovation of that part of its con-
" struction which may be subject to decay? Are
" those powerful operations of *fire*, or *subterraneous*
" *heat*, which so often have filled us with terror and
" astonishment, to be considered as having always
" been? Are they to be concluded as proper to
" *every part* upon the globe, and as *continual* in the
" system of the earth? If these points in question
" shall be decided in the affirmative, we can be at
" no loss in ascertaining the power which has *conso-*
" *lidated* strata, nor in explaining the present *situa-*
" *tion* of those bodies, which had their origin at the
" bottom of the sea. This, therefore, should be the
" object of our pursuit; and in order to have *demon-*
" *stration* in a case of physical inquiry, we must again
" have recourse to the book of nature.

" The

"The general tendency of *heat* is to produce *flu-* "*idity* and *softness;* as that of *cold* is, on the con- "trary, to *harden* soft and fluid bodies. But this "*softening power* of *heat* is not uniform in its nature; "it is made to act very different effects, according "to the nature of the substances to which it is ap- "plied. We are but limited in the art of increasing "the *heat* or the *cold* of bodies; we find, however, "extreme difference in their substances with respect "to *fluidity*.

"A *fusible* substance, or mineral composition in "a *fluid* state, is emitted from those places of the "earth at which *subterraneous fire* and *expansive* "*force* are manifested in those eruptive operations. "In examining these emitted bodies, men of science "find a character for such productions, in general- "ising the substance, and understanding the natural "constitution of those bodies. It is in this manner "that such a person, finding a piece of *lava* in any "place of the earth, says with certainty, here is a "*stone* which has *congealed* from a *melted* state.

"Having thus found a distinguishing character for "those *fused* substances called, in general, *lavas*, "and having the most visible marks for that which "has been actually a *volcano*, naturalists, in examin- "ing different countries, have discovered the most "undoubted proofs of many ancient volcanos, which "had not been before suspected. Thus *volcanos* "will appear to be not a matter of accident, or only "happening in a particular place; they are general

"to

"to the globe, so far as there is no place upon the "earth that may not have an eruption of this kind; "although it is by no means necessary for every "place to have had those eruptions.

"Volcanos are natural to the globe, as genera "operations; but we are not to consider nature a: "having a burning mountain for an end in her inten "tion, or as a principal purpose in forming the ge "neral system of the world. The end of nature ii "placing an *internal fire* or *power of heat*, and : "*force* of irresistible *expansion*, in the body of th "earth, is to *consolidate the sediment collected at th* "*bottom of the sea*, and to form thereof a mass o "permanent *land* above the level of the ocean, fo "the purpose of maintaining plants and animals "The power appointed for this purpose is, as on a "occasions, where the operation is important; an "where there is any danger of a short coming, wisel "provided in abundance; and there are contrive "means for *disposing of the redundancy*. These, i "the present case, are our *volcanos*.

"A volcano is not made on purpose to frighte "superstitious people into fits of piety and devotio "nor to overwhelm devoted cities with destructio: "a volcano should be considered as a *spiracle* to th "subterranean furnace, in order to prevent the ur "necessary *elevation of land*, and fatal effects o "*earthquakes*; and we may rest assured, that the "in general, wisely answer the end of their institu "tion, without being in themselves an end, for whic "natur

" nature had exerted such amazing power and excel-
" lent contrivance.

" Let us take a view of the most elevated places
" of the earth; if the present theory is just, it is
" there that we should find *volcanos*. But is not this
" the case? There are volcanos in the *Andes*; and
" round the *Alps* we find many volcanos, which are
" in France upon the one side, and in Germany up-
" on the other, as well as upon the Italian side,
" where *Vesuvius* still continues to exhibit violent
" eruptions.

" It is not meant to alledge, that it is only upon
" the summit of a continent, *volcanos* should appear.
" *Subterranneous fire* has sometimes made its ap-
" pearance in bursting from the *bottom of the sea.*
" But even in this last case, *land* was raised from
" the *bottom of the sea* before the eruption made its
" *exit* into the atmosphere. It must also be evident,
" that, in this case of the *new island* near *Santorini,*
" had the *expansive power* been *retained,* instead of
" being *discharged,* more *land might have been*
" *raised* above the level of the ocean.

" Now, the *eruption of that elastic force* through
" the bottom of the sea, may be considered as a *waste*
" *of power* in the operations of the globe, where the
" *elevation of indurated strata* is an object in the
" exertion of that *power*; whereas, in the centre of
" a continent sufficiently elevated above the level of
" the sea, the eruption of that *fiery vapour* calcu-

" lated to elevate the *land*, while it may occasionally
" destroy the habitations of a few, provides for the
" security and quiet possession of many.

" In order to see the wisdom of this contrivance,
" let us consider the two extreme places at which
" the eruption of ignited matter may be performed.
" These are, on the one hand, within a continent of
" land, and on the other, at the bottom of the ocean.
" In the one case, the free *eruption of the expanding*
" *power* should be permitted; because the purpose
" for which it had been calculated to exist has been
" accomplished. In the other again, the free erup-
" tion of that powerful matter should be repressed;
" because there is reserved for that *power* much of
" another operation in that place. But according to
" the wise constitution of things, this must necessa-
" rily happen. The eruption of the *fiery vapour*
" from *volcanos* on the continent of land, is inter-
" rupted only occasionally, by the melted bodies
" flowing in the subterraneous chimney; whereas at
" the bottom of the ocean, the contact of water ne-
" cessarily tends to *close the orifice*, by accumulating
" condensed matter upon the weakest place.

" If this be a just theory of the natural operations
" of the globe, we shall have reason to expect, that
" great quantities of this *melted matter*, or *fusible*
" *substance*, may be found in form of *lava*, among
" the strata of the earth, where there are no visible
" marks of any *volcano* or burning mountain, having
" existed. Here, therefore, is an important point to
" be

"be determined; for, if it shall appear that much "of this melted matter, analogous to *lava*, has been "forced to flow among the *strata* which had been "formed at the bottom of the sea, and are now "found forming dry land above its surface, it will "be allowed, that we have discovered the secret "operations of nature *concocting* future *land*, as "well as those by which the present *habitable earth* "had been *produced* from the bottom of the abyss. "Here, therefore, we shall at present rest the ar-"gument, with endeavouring to show that such is "actually the case."

200. This is all that relates to the theory itself: for I shall shortly make it appear that Dr Hutton here rests the argument on a point, which it was unnecessary to prove, but which, in fact, is of no service to him. This passage affords a general example of the philosophical tone, the display of method, and the frequent reference to *final causes*, which characterise his work. It is this method, to which, no doubt, he conceived himself to have scrupulously adhered, which persuaded him, and may persuade many of his readers, that, without the necessity of minutely surveying all the objects presented by the earth's surface, or even of going out of his own cabinet, a "*table of Sicilian marble*," or "*jasper*," and some other specimens brought forward from his collection of minerals, might lead to the formation of a *Theory of the Earth*. In this exposition of the system may be seen also an instance of what I have re-

marked above;—that, when Dr. Hutton was investigating the causes which might have consolidated our mineral strata, he had in view, in their application, his own hypothesis only respecting *sediments of rivers* collected at the bottom of the sea; while I have proved that no such *origin* can possibly be assigned to the materials of which our strata are composed. But what it is at present most important to consider in this exposition is, that Dr. Hutton here explains himself as to the nature of the "expansive "power of heat," supposed to have raised up our continents, an explanation which Mr. Playfair has omitted to give. This power, according to him, is exercised by an *expansible fluid*, which he calls "*fiery vapour*," and this indeed is the only form in which *heat* can be employed in such an operation. Here then we have a fixed point to direct us in the examination of the theory of *elevation*, not only in this system, but in all those which ascribe to this cause the retreat of the sea from our present continents. I shall first however make another observation on the foregoing statement of Dr. Hutton.

201. We have seen him present his theory as leading to two conclusions, which are to be such, that, if verified by facts, they will render it evident, that he has "discovered the secret operations of nature "*concocting* future *land*, as well as those by which "the present *habitable earth* had been *produced* "from the bottom of the abyss." The first of these conclusions, and its proof, are thus expressed. "If "the theory is just, it is in the most *elevated* places

"of

"of the earth that we should find *volcanos*. But is "not this the case? There are *volcanos* in the *An-* "*des*, and we find many round the *Alps*." This is true; but it will appear that these *facts*, instead of confirming his theory, do, on the contrary, overthrow it.

202. The other conclusion, and that on the verification of which he finally rests the whole of his argument, is as follows: "If this be a just theory "of the natural operations of the globe, we shall have "reason to expect that great quantities of this *melted* "*matter* or fusible substance may be found in form "of *lava*, among the *strata* of the earth, where there "are no visible marks of any *volcano*, or burning "mountain, having existed." He then goes on to prove that *basalt* and *whinstone*, those mineral substances on the origin of which mineralogists are divided, are in fact materials, which, in the state of *fusion*, have been injected among the strata; but this is perfectly unconnected with the object which he has in view. Whether these substances are or are not what he supposes them, it is certain that while our mineral strata were covered by the sea, real *lavas* flowed over them, issuing from openings, on which rose considerable *volcanic cones;* in the same manner as, when the sea had abandoned our present continents, a great number of *volcanic islands* were soon formed on its new bed. In my first geological work, I have indicated several of these *lavas*, which had been diffused over the bottom of the ancient sea, and there covered by other *strata*. This, then, would

would answer Dr. Hutton's purpose, if any thing could; but it will be seen, by the discussions upon which I am now about to enter, that this very phenomenon will tend to the subversion of the hypothesis of *elevation*, as well as of that of the *consolidation* of our *strata* by *heat*.

203. To this end, I shall begin with examining what Dr. Hutton adduces as an instance of an *elevation of land*. "It must be evident," he says, "that in the case of the *new island* near *Santorini*, "had the *expansive power* been *retained*, instead of "being *discharged*, more *land* might have been "raised above the level of the ocean." He afterwards, however, observes that, in such a case, "the "free eruption of that powerful matter should be "*repressed*, because there is reserved for that *power* "much of another operation in that place:" (the *elevation* of future continents.) "But according to "the wise constitution of things this must naturally "happen......At the bottom of the ocean, the contact "of water necessarily tends to *close the orifices*, by "accumulating condensed matter upon the weakest "place." This is considered by Dr. Hutton as evident; and so it may have appeared to those who, like him, are unacquainted with the history of the island in question, which arose in the Archipelago in 1707, a fact attested by eye-witnesses: I had related it in the 1st vol. p. 506, of my *Lettres sur l'Histoire de la Terre et de l'Homme*, on the authority of LAZZARO MORO, who, like Dr. Hutton, had cited it in proof of the elevation of our continents; but I showed that it bore no analogy to such an effect.

effect. I shall again insert the account of it here; as it will serve to overthrow every thing that Dr. Hutton has said on the subject.

"Some shocks of an earthquake were first felt in "a neighbouring island called *Santorini;* the sea be-"came tinged with sulphur for twenty miles round; "mineral substances issued from its bottom, which "were forced up to the surface of the water; a "*rocky shelf*, black and tremulous, then arose: be-"tween it and the little island *Cameni*, a *ridge of* "*similar rocks* was next formed, and by degrees the "several masses united together. As the island be-"came more extensive, the smoke increased, and "*flames* appeared. When the island had acquired "a circumference of six miles, and an elevation of "one hundred feet, no less than sixty *orifices* were "observed in it, vomiting forth smoke, flames, and "ignited stones; which latter, as they accumulated, "gradually raised the island to a greater height. "This operation continued for the space of four "years, when the *explosions* at length ceased; and "thus was formed an island composed of *lava, cin-"ders*, and *pumice stone.*"

204. Is this an *elevation of land*, i. e. of *mineral strata* similar to those which form the mass of our continents? Yet such it ought to be, if adduced as an instance of *elevation* in any way connected with the present subject. After having brought forward, in my former work, these details of the operation, I compared it, and even the formation of the highest *volcanic*

volcanic cones, to the work of moles in our meadows, which also form *cones*, by throwing up the earth through an opening of the surface. Here the acting power, that expansible fluid called "*fiery vapour*" by Dr. Hutton, forced up, through sixty different orifices, "*lavas, cinders, and pumice stone.*" These, indeed, produced an *island;*—but no *elevation of land* took place; nothing was forced up but these loose materials.

205. We also find from this account that the operation did not end in the manner supposed by Dr. Hutton; its *cessation* not having arisen from the "*closing of the orifices by matter condensed*" by the "*contact of water;*" for so long as the operation was confined below the surface of the water, the contact with that liquid produced only the decomposition of the expansible fluid, which gave to it a "sulphureous tinge," in like manner as the same fluid deposits sulphur in the craters and other openings of the *Lipari Islands:* but it still preserved channels for itself through the substances which it propelled from the openings; and when these substances had reached the surface of the water, the sixty chimnies, on attaining that elevation, became so many volcanic mouths, which at last ceased to vomit. It was not, therefore, because the orifices were "*closed*" that the operation ceased; it ceased without, only because it had already ceased within.

206. All similar operations, even those which have produced the most elevated volcanic cones, consist

only

only of materials thrown out, through an opening, on the surface, where they spread themselves over an existing basis, extending in proportion as the chimney, prolonged in the axis of the increasing cone, is carried up to a greater height. But it often happens that this basis, undermined by the same operations that produce the lava, becomes too weak to support the mass of the cone, which then sinks in. Such has been the case with Mount *Vesuvius*, which was formerly much higher than it is at this day. The present Vesuvius is a new cone, raised upon the ruins of the ancient, of which *Monte Somma* is a remaining part. By the same catastrophe, as mentioned in my work, were destroyed immense cones, while still at the bottom of the sea; the sections only remaining of their latest lavas, which had spread to a considerable extent, and were afterwards covered by other strata. I described several of these sections of lava in Germany; which, being turned towards a central point, and forming circular ridges around an empty space, I called *volcanic crowns;* and I compared them to the relics of large trees in forests, hollowed out by time, and exhibiting only such sections of their decayed trunks, of which the roots extend under the surface of the soil, as resemble these remains of lavas around the depressed cones of volcanic mountains. The place of these cones is generally occupied by a lake, unless the waters find there some outlet, either natural or artificial.

207. Thus, from the formation of the *Isola nuova*, in the Archipelago, from that of *Monte nuovo* near Naples,

Naples, (an account of which I had also given from Lazzaro Moro, in my *Lettres sur l'Histoire de la Terre et de l'Homme;*) from my brother's very accurate observations on *Etna, Vesuvius,* and the *Lipari Islands,* likewise inserted in my work; and from my own observations on the *ancient volcanos* of Germany; it is evident that the "*power*" to which the *elevation of our continents* has been ascribed, namely, that of *expansible fluids* formed in the interior parts of the globe, never acted, nor indeed could act, as an instrument of *elevating* any materials, without forcing its way through some fissures of the strata; where, while it drove before it substances in *fusion,* it must of necessity have escaped. It is no less evident, from the instances to which I have now referred, that when, by the means of the same operations which produce lavas, the supports of the masses of strata, upon which large volcanic cones have been formed by the accumulation of lavas and cinders, are undermined, these cones sink down, their basis having given way. Had, therefore, these undoubted facts been attentively considered, how could it for a moment have been conceived, that such *vaults* as our continents could have preserved their stability over *caverns* of equal extent, formed in the very act of "*elevation?*"

208. At the time when Lazzaro Moro and other geologists had recourse to such an operation, in order to account for the emersion of our continents from the bosom of the ocean, it was not known that they were only vast heaps of *ruins.* No attempts had

yet

yet been made to ascertain the cause of the highly *inclined* position of the strata, not only in the *mountains*, but in *hills*, and even beneath the *plains*; and consequently no idea had been formed of the *angular motions* of enormous masses of strata, by which that state had been produced: in a word, all that naturalists had in view was to explain the *elevation* of a continuous mass. However, when I first refuted that theory of elevation, though I had by no means then acquired sufficient knowledge of the great chains of mountains, I clearly saw that the idea was a mere illusion. I did not deny to the expansible fluids the power of raising up the entire mass of our continents; but it was already evident that these fluids could not have detached such a mass from the earth, without producing in this mass innumerable fractures; and that, after they had escaped through the openings, the elevated mass must have fallen back into the depths of the ocean.

209. I was even at that time sufficiently acquainted with the state of the strata which we now denominate *secondary*, to be aware that they had undergone great catastrophes; and for the same reason which I have just alleged, I attributed these catastrophes to the *subsidence* of the parts now comparatively the lowest, and the emersion of our continents to the *sinking* of others, which the sea had covered in its retreat. I then remarked, that, as both theories equally assumed the existence of *caverns*, it was much more conformable to the nature of things, that these caverns should have preceded and occasioned the

the subsidences, than that they should have been formed during the process of elevation. For, in the first case, the masses which had *sunk down* coming to rest on the bottom of the caverns, every thing would remain in a state of stability; whereas, in the latter, the *fractured continents* must have remained suspended over *caverns*, which is manifestly impossible.

210. But since that period, the impossibility has been rendered more obvious by M. de Saussure's discoveries relative to the great chains of mountains. Guided by these in the course of my more recent observations, and directed in my physical researches by his first opinions respecting a succession of different *chemical precipitations* from the same *liquid*, as well as by the general progress of chemistry, I was at length enabled to determine the cause of the commencement of all the operations, the monuments of which we observe on our globe, in the manner developed in my preliminary discourse, § 60. During the prosecution of my researches, having conceived the cause of the successive formation of caverns under the strata, I found in the same cause that of a *succession of different precipitations*; and thus, by their relation to a common cause, were connected two great events, the succession of the strata, and the series of catastrophes which they underwent. Moreover, according to my theory of subsidence, these catastrophes could not affect the stability of of those vast ruins; while, according to the opposite theory,

theory, the result would have been a mere repetition of *elevations*, soon followed by *depressions*.

211. I developed my ideas on this subject, for the first time, in my letters to M. de la Métherie, published in his *Journal de Physique*, in 1790 and 1791. I gave an abridgment of my system in my first letter to Dr. Hutton, published in the *Monthly Review*, 1790; and I more fully explained it in my letters to Prof. Blumenbach, inserted in the *British Critic*, 1793 and 1794. This Mr. Playfair might have known; but in § 419 of his work, he alleges the following as the reason of his not knowing it. Speaking of my tenth letter to M. de la Métherie, he thus expresses himself: "I must confess that I am "unacquainted with every thing of this letter but the "*title;* and could not easily be prevailed on to fol-"low any man who professedly goes out of nature "in search of knowledge; who pretends to give the "*history of our planetary system* when there was "*no sun*, and to enumerate the events which took "place between the *existence of that luminary, and* "*the existence of light*. The absurdity of such an "undertaking admits of no apology; and the smile "which it might excite, if addressed merely to the "fancy, gives place to indignation when it assumes "the air of philosophical investigation."

212. I shall not here accuse Mr. Playfair of any thing more than an astonishing degree of inattention, arising from his attachment to his friend's system. He evidently thinks himself justified in what he says; for

for he afterwards very correctly translates the *title* of the Letter, on which alone he grounds his remark*; and he even gives it in a note in its original language, which is as follows: "Sur l'Histoire de la Terre, "depuis que cette planete fut pénétrée de *lumière*, "jusqu 'à *l'apparition* du Soleil: espace de temps "qui renferme les origines de la chaleur, de la figure "de notre globe, de ses couches primordiales, de "l'ancienne mer, de nos continens comme fond de "cette mer, de leurs grandes chaînes de montagnes, "et de la végétation." If Mr. Playfair's indignation, excited by his own misconception of my meaning in this *title*, had not withheld him from perusing the *letter*, he would soon have been aware of the express distinction which I made between the *appearance* of the sun, and its *existence;* since I considered it as a *central body* in our planetary system from its origin. He would next have seen the particular functions which I assigned to *light*, in the physical operations of the universe, including those which were to take place in bodies formed to be *phosphoric*, before they should become so; and he would have perceived that, far from "addressing myself to the *fancy*," I pursued a didactic method for such persons as might be disposed to give me that attention, which he has not been willing to bestow on the letter in question.

* "On the History of the Earth, from the time when that "planet was penetrated by light, till the *appearance* of the "sun; a portion of time which includes the origin of heat, and "of the figure of the earth; of its primeval strata, of the an- "cient sea, of our continents as the bottom of that sea, of the "great chains of mountains, and of vegetation."

With

ith regard to his remark, that I go "*out of na-ture* in search of knowledge," this alludes to what said concerning the *origin* of light; now, if he can cover it *in nature*,—"*dicat.....et erit mihi magnus Apollo.*" I trust that this explanation, by con-ıçing him into what erroneous judgments we may betrayed from want of attention, will induce him grant some to what I am at present stating.

213. Although neither Mr. Playfair nor Dr. Hut-n explain their object, in supposing a large class *unstratified substances*, produced by *fusion*, un-rneath the mass of strata, their purpose evidently to *close the openings* made by the rupture of the rata, and more particularly to *fill up* the spaces cessary, during the process of elevation, to the gular motions of their separated masses; through iich passages the *expansible fluid* would have made escape. Now towards this end, it answered no rpose to fill up the *mineral veins*, on which great ess is laid; for the *fissures* wherein they were rmed are nothing, in comparison with the *intervals* pable of admitting angular movements. Neither is it of any greater avail to assimilate basalt and d whinstone to lava; for the strata of those sub-ances have suffered all the catastrophes of the hers. It was, therefore, without doubt, for this ason, that Dr. Hutton had recourse to *granite*. justly considered it as lying *under* all the other ineral substances; and he thence imagined, that, it were a product of *fusion*, and had been raised der all the other strata, it must have filled up all their

their interstices; and that then, becoming indurated in cooling, it would have *cemented* the whole of the vault. He has not explained himself on this subject; yet such must have been his opinion, since he relies on the solidity of the vault so far as not to be apprehensive of the *openings* through which the "*fiery vapour*" escapes, but even, on the contrary, to adduce them in proof of his system. "If the "theory is just," he says, "it is in the most elevated "places of the earth that we should find volcanos. "But is not this the case?.....The free *eruption of* "*the expansive power* should be permitted, because "the purpose for which it had been calculated to "exist has been accomplished." But, in adverting above, § 201, to this pretended *proof*, I announced that these facts would appear to be in reality subversive of the theory; and this I shall now proceed to show.

214. The substances in fusion must form and accumulate on some basis, before they can be conveyed into the volcanic channels. This cannot take place on an inverted basis, such as the interior surface of the vault; besides, lava is not granite, and it is with granite that this under part of the elevated strata is supposed to be covered. Thus the materials of lava can form and accumulate only on the *bottom of the caverns;* and therefore the height to which it must ascend, before it can reach the chimney of a volcano, is no less than that of the whole space left empty by the elevation of the strata. Now no effort of the imagination can suggest a "*power*" capable of

of throwing up *insulated columns of this melted substance*, exactly opposite to the openings of the channels formed in the vault, and of continuing to force them through these channels up to the surface. The whole *theory of elevation*, therefore, is overthrown by the very *fact* adduced in proof of it by Dr. Hutton; and this operation being so peremptorily excluded, the theory of subsidence only is left for the explanation of the very phenomenon which he had assumed as a proof of his own. I had applied this theory, in my first geological work, to the accurate observations of my brother on an eruption of *Vesuvius*, and on the island *Vulcano*, one of the Lipari Islands; and I shall here briefly recapitulate what I said on the subject.

215. During the subsidences of the strata, the fractured masses sunk down into the caverns by which the disruptions had been caused; and as the formation of new caverns occasioned repetitions of these catastrophes, the masses sunk still lower, leaving between them interstices like the cavities which we observe in the internal parts of our mountains, and under some plains, but deeper and of much greater extent; as plainly appears from earthquakes. Lavas are certainly foreign from our strata; this is acknowledged by Dr. Hutton: they are formed under all the strata; for volcanos now and then throw up fragments of *granite*, some of which my brother has met with on Vesuvius. Besides, it requires the resistance of the whole mass of our strata, to bear the re-action of the *expansible fluids*, when

they throw lavas up to the summit of *Etna* and the *Andes*. The lavas, then, are formed at the bottom of *caverns*, from mineral substances, unknown to us in their natural state. When these substances have accumulated in those subterraneous galleries in a sufficient quantity to obstruct the lower part of such continuous but tortuous channels as I have found in some extinguished volcanos in Germany, offered them by the interstices of the masses of strata, the confined expansible fluids acquire the degree of density requisite to force up the lavas through these channels, which are gradually prolonged in the volcanic cones. If the passage becomes obstructed, by the remains of preceding lavas cooled and hardened within it, the new lava which has already entered this channel, is stopped in its course. This state is indicated by the smoke ceasing to issue from the crater; and also by earthquakes, which are the natural result of the confinement of the expansible fluid; and the reason of their being seldom attended by considerable catastrophes, is found in the theory of subsidence, according to which the masses of strata undergoing these tremulous motions rest on the bottom of the caverns; while in the theory of elevation, such convulsions, occurring in a broken vault, would, notwithstanding the cement of granite, inevitably cause it to fall in. The earthquakes, thus occasioned by the obstruction of some *volcanic spiracle*, continue until the heat of the new lava has mollified the remains of the preceding lava. As soon as this happens, the *expansible fluids* raise the whole, and produce an eruption, which often breaks out in some

part

part of the sides of the cone, if the ordinary passage is not entirely freed. At such times the most considerable explosions of cinders, and even of large masses, takes place from the crater; because the expansible fluids penetrate the column, and rush out at its upper extremity; a circumstance observed by my brother, while standing on the very brink of the crater of Vesuvius, during an eruption.

216. In this comparison of the theory of *subsidence*, with that of *elevation*, it has not been necessary to enter upon the question, whether *granite* is a product of *fusion*, and thus *unstratified*. But, after having shown that this hypothesis is of no avail to the Huttonian theory, I shall now consider it as a separate object, on account of its importance, not only in mineralogy, but, more particularly, in *geology*. It is important to the latter science, because of the theory of Dr. Hutton, who denies the possibility of discerning any marks of a *beginning*, in the operations of which we see the monuments upon our globe; and it is no less essential when considered in reference to those, who think that geology is not yet, nor perhaps ever will be, sufficiently advanced to ascend to such a beginning in the history of the earth, and thence to trace down that history to our own time. If granite is a product of *fusion*, Dr. Hutton is authorised to think that every thing antecedent to its formation is a mere subject of conjecture, and it is on this account that he supposes a succession of continents, some destroyed, and others elevated, without our being able to discover how this

succession first began. But if *granite is disposed in strata*, if, like other strata, it is the result of *chemical precipitations*, its production is then to be considered as the *first effect of physical causes* on our globe. I have shown in the preliminary discourse, § 57, how this point alone, when properly investigated, determines the whole history of the earth; and the circumstance of having to establish it against an opposite theory may be regarded as favourable; since truth is ever rendered more conspicuous by the objections raised against it. With this view, I shall return to Mr. Playfair's work.

217. I have already had occasion to remark, that, in the Huttonian theory, the same circumstance is brought forward to prove *granite* to be a product of *fusion*, which had led some eminent mineralogists to consider it as the result of *chemical precipitations*; namely, that it is composed of a number of small *crystals* of different kinds. We shall presently find that M. de Saussure, who is in the first rank among geologists, adduces facts in confirmation of the opinion which I here follow; whereas none are presented in support of the hypothesis of fusion. We have seen that Dr. Hutton grounds himself on the phenomenon of lavas, the only product of fusion in the subterraneous regions, of which we have a direct knowledge; but were we, with him, to add to lavas basalt and whinstone, we should still find nothing that at all resembled *granite*. After having given in the text this singular proof, derived from the crystals of the granite, Mr. Playfair resumes the subject in his notes,

p. 329.

p. 329. He there begins by acknowledging that several mineralogists have brought forward instances to prove the *stratification* of granite, and that he himself has observed two similar examples; after which he thus proceeds:

"What exists in two instances, may exist in many; "and, after these observations, I should be guilty "of great inconsistency, in refusing to assent to the "accounts of Pallas, De Luc, Saussure, and many "other mineralogists, who so often represent *granite* "as formed into *strata.* In some cases, however, "it is certain, that the *stratification* they describe is "extremely unlike that in the two instances just "mentioned, and indeed very unlike any thing that "is elsewhere known by the name of stratification. "For example, the stratification must be very am- "biguous, and very obscurely marked, that was not "discovered till after a series of observations, con- "tinued for more than twenty years by a very skilful "and distinguishing mineralogist. Yet such un- "doubtedly is the *stratification of Mont Blanc,* and "of the *granite* mountains in its neighbourhood, as "it escaped the eyes of Saussure, in the repeated "visits which he made to them, during a period of "no less extent than has just been mentioned. It "was not till near the conclusion of those la- "bours, to which the geologists of every age will "consider themselves as highly indebted, that, hav- "ing reached the summit of Mont Blanc, he per- "ceived, or thought that he perceived, the *stratifi- "cation* of the *granite* mountains. The *Aiguilles*

or

"or Needles which border the valley of *Chamouni;* "and even Mont Blanc itself, appeared to be formed "of vast tabular masses of *granite,* in position nearly "vertical, and so exactly parallel, that he did not "hesitate to call them by the name of *strata.* Till "this moment, these same mountains, viewed from "a lower point, had been regarded by him as com-"posed of great plates of rock, nearly vertical in-"deed, but applied, as it were, round an axis, and "resembling the leaves of an artichoke."

218. Here again we must examine what M. de Saussure himself says, in those Travels, "to which," as Mr. Playfair justly observes, "geologists *of every* "*age* will consider themselves as highly indebted." But first geologists of the present day would do well carefully to study that work, which contains so many valuable descriptions of important phenomena; for if Mr. Playfair had done so, he could not have said of that eminent mineralogist, that it was not till he "*reached the summit of Mont Blanc,*" that he "*perceived, or thought that he perceived the stra-*"*tification of the granite mountains.*" It was, on the contrary, in 1779, eight years previous to his excursion to Mont Blanc, that he had thus expressed himself in his first volume, § 135. "It will be seen "in the course of this work, that, on attentively "observing the *granite* in the mountains where its "primitive position has not been deranged, it is "found to be in *layers* or *banks,* sometimes indeed "of greater thickness than those in the secondary "mountains, but equally continuous, and disposed

"with

"with nearly the same regularity." Again he says, § 136; "I conceive that the several parts of *gra-nite* are all *contemporary*; that they have been formed in the same element, and by the same cause; and that the principle of this formation has been *crystallization*. Elements of quartz, of schorl, and of feld-spar, dissolved in the same *liquid*, have descended, as they crystallized, to the bottom of the liquid, some separate, some intermixed; in the same manner as we see water, saturated with different salts, deposite, at the bottom of the same vessel, the crystals of all these salts, more or less regularly formed, and more or less interwoven with each other. But I reserve detailed proofs of this explanation, for that part of the work in which I shall treat of granite mountains."

219. Five years before this, M. de Saussure had made his second *Voyage autour du Mont Blanc*, the details of which form a part of his second volume, published in 1786: wherein, speaking of the remarks that he had made during his observation of the granite mountains bordering the *Glacier de Miage*, he says, § 893: "I asked myself, while observing these phenomena, whether the whole of this organisation did not prove a *crystallization*, which had produced at the bottom of the sea *horizontal strata*, afterwards, by some great revolution, set upright upon their edges, and in the course of time divided? Eleven years of subsequent observations have served only to confirm me in this opinion." Now it was not till a year after this, in 1787, that M.

M. de Saussure reached the summit of Mont Blanc: was it then at that time only that he discovered (or, as Mr. Playfair says, " perceived, or thought that " he perceived") the *stratification* of the *granite* mountains? He then, indeed, became sensible of a mistake into which he had fallen, respecting the construction of the *Aiguille du Midi;* but what excites my greatest surprise is that Mr. Playfair, being acquainted with the part of M. de Saussure's work in which he corrects that mistake, should not have found there proofs of the *stratification* of granite, the most unanswerable, and, I may say, the most characteristic of truth. It is one of the most masterly specimens of observation and philosophical discussion to be met with in any geological work; and I doubt not that I shall gratify all those who do not possess this valuable collection of travels, by transcribing the whole of the passage in question. It occurs in the 4th volume, and begins at § 1996.

" Notwithstanding the irregularities observable in " the form and general distribution of the great " masses, I perceived indubitable and important " analogies in the structure of the several parts. " Every thing that I could distinctly see appeared " to me composed of *vertical plates;* and by far " the greatest number of these plates held a direc- " tion from N. E. to S. W. I felt a peculiar plea- " sure in observing this structure in the *Aiguille du* " *Midi.* I related, in chap. XVIII. vol. II. with " how much difficulty and danger I had dragged " myself to the foot of this obelisk, in order to study " its

"its form; and with what regret I had beheld the "inaccessible walls of *granite* surrounding its base, "which opposed themselves to my eager curiosity. "But here I viewed it beneath my feet, and could "minutely inspect it in all its parts. On the second "day of my journey, upon reaching the edge of the "bank of snow where I had passed the night, I saw "to the N. E., at a small distance below me, an "object that exhibited the appearance of dismantled "battlements. I asked *Pierre Balmat* what this "might be; and when he told me that it was the "Aiguille du Midi, which I soon ascertained to be "the case, I experienced a sensation of delight that "I cannot easily express. I did not lose sight of it "as I continued to ascend; and I was soon con-"vinced that, like the *Aiguilles de Blaitières*, it was "entirely composed of *vast plates of granite*, per-"pendicular to the horizon, and holding a direction "from N. E. to S. W. Three of these plates, se-"parated from one another, form its summit, and "others similar to them, decreasing gradually in "height, compose its declivity to the south, on the "side towards *Cramont*."

220. The distinction here made by M. de Saussure, between the position of the great masses, and the structure of the parts, strongly marks the nature of the catastrophes suffered by our strata, which were originally horizontal and continuous. The fractured masses, having undergone angular movements, formed those immense obelisks called *Les Aiguilles*, of which *Mont Blanc* itself, towering in proud pre-eminence above

above the rest, is one. An unequal subsidence of these masses on opposite sides, an effect ascribed by M. de Saussure to what he terms a *refoulement**, which may, to a certain degree, have taken place, produced these angular movements, by means of which the directions and inclinations of the strata may have varied in the great masses. But their structure, that is, their formation in parallel *strata*, must have been similar in all. This circumstance is of importance, as M. de Saussure well observes; since it indicates, both that *granite* was originally formed in *strata*, and that the fractured parts of its mass have undergone angular movements. Now the following is the passage wherein he acknowledges that he had fallen into an error.

"I am therefore led to believe that I was mistaken, "when, on viewing it from a lower point upward, I "judged it to be composed of foliated rock, applied "round an axis, like the leaves of an artichoke; at "least, if there are any plates disposed in this order, "they can be only the lowest, (which, without doubt, "have been displaced;) for, on looking down into "the interior part, I observed that all the *plates* "were perfectly *parallel* to each other. I have given "a detailed description of this summit, as affording "an illustration of the rest: all those which I could "distinctly see exhibited nearly the same form and "the same direction; and if there were any excep-

* See Mr. De Luc's sense of this term, § 221.

"tions,

"tions, they were local and inconsiderable. This
"striking phenomenon is accounted for, as I hope
"to show in my *Theory*, by the lateral pressure,
"(*refoulement*,) which placed upright on their edges
"those *strata* that were originally *horizontal*."

221. Unfortunately, M. de Saussure not having published this Theory, which he promised in every successive volume, we do not know what he meant by *refoulement*. We may suppose, however, that, during the catastrophes suffered by the strata, of which he was the first to discover and establish the proofs, large fractured masses, not subsiding in a parallel direction, nor with the same degree of velocity, reciprocally compelled each other to turn, which is an evident consequence of some lateral pressure. Now we must here recollect, that, in his important remark made on the summit of *Cramont*, as early as in the year 1774, he proved that those phenomena of *granite* were common to it and to the *secondary strata*, proceeding in both from the same cause. Among other observations on this subject, he introduces the following, in the chapter wherein he speaks of Cramont, § 919. "The many proofs which, at
"the time when I made that remark, I had before
"my eyes, and others analogous to those, which my
"memory readily suggested, induced me to presume
"its universality; and I immediately connected it
"with the observations which I had just made on
"the structure of Mont Blanc, and of the primitive
"chain of which it forms a part. I saw this chain
"composed of *plates*, which might be considered as

"*strata;*

"*strata*;—I observed these strata, *vertical* in the "centre of the chain, and the *secondary strata*, "nearly *vertical* in the line of their junction with "them, but becoming less so at a greater distance, "and gradually approaching a *horizontal* position, "in proportion as they receded from their point of "support. I also saw the gradations, which I had "already observed in the *materials* of the primitive "and the secondary strata, extending likewise to the "*form and position of the strata* themselves; as all "the summits of the *secondary*, which I viewed be-"neath me, terminated in sharp pyramidal plates, "like *Mont Blanc*, and the mountains that compose "its chain." When facts so important, so precise, and so evident, are brought forward, how can an origin be ascribed to *granite* different from that of all the other *strata* which follow it in those mountains? The inference, therefore, drawn by M. de Saussure, has indeed a strong claim upon the attention of "geologists of every age." "I conclude," he says, "from all these manifest relations, that "since the *secondary* strata were formed at the bot-"tom of the sea, it follows that *the primitive must* "*have had a similar origin.*" I shall now return to the remarks of this able naturalist, as contained in that very passage, § 1997, from which Mr. Playfair inferred only that he "perceived, or *thought* that "he perceived, the *stratification* of the granite "mountains;" while, on the contrary, he there brings in proof of it a striking and demonstrative argument.

"But

"But another question," he says, "which I ardently wished to resolve, was whether these vast "*plates* preserved the same nature from their *bases*, "with which I had been long acquainted, up to their "*summits*, which I had not as yet closely inspected. "I was completely satisfied; I found the *summits* of "these peaks, as well those which we were able actually to reach, and of which a description is given "in the preceding chapter, as those sufficiently near "to afford us an opportunity of distinctly ascertaining the substances of which they were composed, "to be unquestionably, like their bases, formed of "*granite*, *granitelle*, *veined granite*, and other stones "of the same class.

"This fact is so important to theory, that, although I had observed it on less elevated mountains, and had supposed the case to be most probably the same with others, I felt extreme satisfaction in generalizing it by a direct observation. "In truth we thereby ascertain a very remarkable "property of mountains composed of *vertical strata*; "viz. that their nature is the same, from their base "to their summit. (Note.—It is essential to observe, that this identity can be understood only of "a section, vertical, or parallel to the strata; or, "which comes to the same thing, of one particular "*stratum*, the lowest part of which is compared "with the most elevated. For, if we viewed the "section of a mountain intersecting the *strata* at "right angles, or even obliquely to their direction, "then, in ascending, we should meet with different

"strata,

" strata, and it might happen that we should find " rocks above of a different class from those below.)— " In those mountains, on the contrary, in which the " strata are *horizontal* or nearly so, we find them " vary, in the same vertical section of the mountain, " as we ascend. The *Buet* rests on a base which is " *primitive*, while its summit is *secondary*. The " mountain of *Forca del Bosco* has its base of hard " *granite*, veined, and coarse grained, and, as we " ascend, we see the *granite* degenerate into *foliated* " rocks of a very different kind. We shall find that " the same observation will apply to *Mont Rose* and " *Mont Cervin*.

" This difference results from the different actions " of the cause which has given to these various clas- " ses of mountains their present form and position. " In those which are composed of vertical plates, " each plate is one and the same *stratum*, in the strict " sense of the word, and not the product of any ac- " cidental *fissure*, as some naturalists have imagined. " These strata were originally horizontal, and it is " in consequence only of a revolution on the globe " that they are now placed on their edges. It is " natural, then, that each of them, throughout its " whole height, should preserve the identical cha- " racter which it had at its formation. Those moun- " tains, on the contrary, which are composed of *ho-* " *rizontal* divisions, acquired their height by the " simple accumulation of different *strata*, consisting " of crystallizations and deposites, the nature of " which varied, according to the materials contained " in the waters where they were formed."

222. I

222. I had already adopted M. de Saussure's opinion respecting the nature of granite from the passages above quoted, from his first volume, before this masterly demonstration of it had appeared. I had been one of those who, as he says, had mistaken for *fissures* the very numerous *vertical lines* observed in the abrupt lines of *schistose* as well as of *granitic* mountains; and this error had effectually prevented me from understanding the mountains of that class; as I acknowledged in my first geological work. But on the publication of M. de Saussure's second volume, containing not only the chapter on the Cramont, whence I have extracted the passage above cited, but likewise a very clear general definition of the peculiar characters by which it might be ascertained, whether these lines were divisions between distinct strata, or between parts of one and the same mass in which *fissures* had been made, I did not hesitate to admit his opinion, that all our *mineral strata, beginning with granite, had been produced by chemical precipitations from a liquid which had originally encompassed our globe*. And nothing, I should conceive, but Mr. Playfair's prepossession in favour of his friend's system, can have made him find in M. de Saussure's work, that twenty years had elapsed before he discovered the *stratification of granite*, on his visiting Mont Blanc. This prejudice, however, has influenced Mr. Playfair's judgment, on another very important subject, to which I am now coming, and in which he sets out from the opinion that the granite of that mountain is *not stratified*: an idea so evidently at variance with facts, that those notions

notions of his, which I shall here examine, respecting the formation of chains of mountains, might hence alone be overthrown; but the subject is too important in itself to be considered in that single point of view.

223. In my first Letter to Dr. Hutton, where I controverted his opinion that granite was a product of *fusion*, I adduced against it, among other objections, the following, § 30: " If a substance in *fu*" *sion* had burst through our strata, would it only " have filled up their *fissures?* Would it not have " diffused itself in torrents on the surface, like the " *lavas?*" Mr. Playfair, who mentions none of my objections, has doubtless not attended to this; but it was made also by another mineralogist, whom he probably deemed it necessary to answer; and that answer will furnish me with a subject of farther remarks. It begins, p. 340.

" An argument, directed at once against the *igne*" *ous origin* and *unstratified* nature of all *granite*, " is given in a work already mentioned. (*Mineral*" *ogy of the Scottish Isles*, vol. II. p. 166.) If gra" nite had flowed from below, how does it happen, " that, after it had burst through the strata of mi" caceous schistus, &c. it did not overflow the neigh" bouring country? If this hypothesis were true, " *Mont Blanc* could never have existed." " A the" ory is never more unfairly dealt with, than when " those parts are separated which were meant to sup" port one another, and each left to stand or fall by

 " itself.

"itself. This, however, is precisely what is done
"in the present instance; for Dr. Hutton's theory
"of *granite* would not deserve a moment's conside-
"ration, if it were so inartificially constructed, as to
"suppose that granite was originally fluid, and yet
"to point out no means of hindering this fluid from
"diffusing itself over the strata, and settling in a
"horizontal plane. The truth is, that his theory,
"at the same time that it conceives this stone to have
"been in fusion, supposes it to have been, in that
"state, injected among the strata already consoli-
"dated; to have heaved them up, and to have been
"formed in the concavity so produced, as in a mould.
"Thus Mont Blanc, supposing that it is *unstrati-*
"*fied*, is understood to consist of a mass that was
"melted by subterraneous heat *under the strata*,
"and being impelled upwards by a force, that may
"stand in some comparison with that which pro-
"jected the planets in their orbits, heaved up the
"strata by which it was covered, and in which it
"remained included on all sides.

"The covering of strata, thus raised up, may
"have been burst asunder at the summit, where the
"curvature and elevation were the greatest; but the
"*melted mass* underneath may have already acquired
"*solidity*, or may have been sustained by the beds
"of *schistus* incumbent on its sides. This schistus,
"forming the exterior crust, was immediately acted
"on by the causes of waste and decomposition,
"which have long since stripped the *granite* of a
"great part of its covering, and are now exercising

" their power on the central mass. That even Mont
" Blanc itself, as well as other *unstratified* moun-
" tains, was once covered with schistus, will appear
" to have in it nothing incongruous, when we con-
" sider the height to which the schistus still rises on
" its sides, or in the adjacent mountains; and when
" we reflect, that, from the appearances of waste and
" degradation which these mountains exhibit, it is
" certain that the *schistus* must have reached much
" higher than it does at present.

" It is obvious, therefore, that when the corre-
" sponding parts are brought together, and placed
" in their natural order, no room is left for the re-
" proach, that this system is inconsistent with the
" *existence of granite* mountains. I have no plea-
" sure in controversial writing; and, notwithstanding
" the advantages which a weak attack always gives
" to a defender, I cannot but regret, that Dr. Hut-
" ton's adversaries have been so much more eager to
" refute than to understand his theory."

224. Mr. Playfair takes no notice, in this passage, of the objections brought against the theory of *elevation;* he thinks only of preventing the *extravasation of the granite,* and for that purpose he envelopes it in the *schistus.* I shall soon return to this subject; but I must first observe that, while he accuses those who do not acquiesce in Dr. Hutton's theory of being " *much more eager to refute than* " *to understand it,*" it is himself, on the contrary, who here deserts that theory, and even exposes it to ridicule.

ridicule. This hypothesis of Dr. Hutton respecting granite, which alone here engages his attention, forms only an appendage to that of *elevation*; the "*melted* "*granite*" itself requiring to be heaved up under the strata. The objection, which I long ago adduced against the theory of elevation, was, that the only manner in which the action of *heat* could be employed for this purpose, was by the production of some *expansible fluid;* and that, when this fluid had *escaped* through the fractures of the mass of our continents, that mass would have fallen back again; a circumstance which granite in a state of *fusion*, flowing all around down the sides of the vault, could not have prevented. Mr. Playfair eluded this objection, by not explaining himself with regard to the mode in which heat was supposed to exercise its "*expansive power*:" I have therefore reverted to Dr. Hutton's own exposition of his theory, and there we have seen him ascribe that effect to the expansible fluid, which he calls "*fiery vapour*." This formed the main objection urged in my first Letter; to which I added only as accessory that relating to the extravasation of the supposed melted granite. Mr. Playfair, doubtless, well understood the force of the former objection; since, although, on account of the latter, he was obliged to point out a *power* that should have been capable of *moulding* the *granite* in the *schistus*, while it heaved them up together, he did not venture to adduce the "*fiery vapour*" of Dr. Hutton. But what *force* has he employed? "A '*force*," he says, "that may stand in some compa-" "rison with that which projected the planets in their

 "orbits."

"orbits." Can this be called an *illustration* of Dr. Hutton's theory?

225. This, I apprehend, will suffice as to the reproach, that the opponents of Dr. Hutton's theory "*have been more eager to refute than to understand* "*it*;" but it is of importance to examine Mr. Playfair's mode of vindicating that theory against the second of my objections, in the foregoing passage, where it is developed. He supposes that the region of the Alps was occupied, at the birth of our continents, by one *continuous* mass, more elevated than Mont Blanc is at present; and it must have been the same with all other *mountainous* tracts; since this theory, in which the angular movements of the great masses are admitted, supposes also, in common with the systems of less advanced geologists, that the *vallies* by which these tracts are divided into chains, have been *excavated* by *running waters*. Here then we are brought back to this great geological object; for, in the theory which we are discussing, every means must have been employed to support the hypothesis of the hollowing out of vallies, because they are required to furnish *materials* for *future continents:* and we are brought again to this point more particularly in reference to one of equal importance, which I have for some time kept back, on account of the many others that were to be connected with it; I mean the *blocks* of *granite*, and of other kinds of *stone*, which are found disseminated over the surface of our continents.

226. Little

226. Little attention has hitherto been paid to this important phenomenon, on account of the vague idea, that, in the course of *numberless ages*, these blocks, propelled by running waters, may have travelled over our continents. I have never refused *time* for the production of any *effects* to which a *cause* has been assigned, examining only whether the cause adduced were adequate. Now, I have long opposed to the action of running waters in the case here in view, the interposition of vast *chains of mountains*, between many tracks over which these blocks lie scattered and every eminence composed of the same kind of stone. Dr. Hutton and Mr. Playfair are the only geologists who have noticed this objection; and they have attempted to obviate it, by supposing the migration of these blocks to have *preceded* the hollowing out of the vallies by running waters. A full discussion of this only resource of the hypothesis of such a migration will be very useful to those who take interest in geological subjects, by evincing the importance of a phenomenon so common as that of the scattered blocks, in an inquiry into the *causes* which acted on our globe, before the birth of our continents. In order to introduce the subject here, I shall revert to a passage of Mr. Playfair, which I cited before, § 113, and which is in § 112 of his own work.

" The operation of rains and torrents," he says, " modified by the hardness and tenacity of the rock, " has worked the whole into its present form; has " *hollowed out the vallies*, and gradually detached the

" the *mountains* from the *general mass*, cutting down
" their sides into steep precipices at one place, and
" smoothing them into gentle declivities at another.
" From this has resulted a *transportation* of *materi-*
" *als*, which, both for the quantity of the whole,
" and the magnitude of the individual fragments,
" must seem incredible to every one, who has not
" *learned to calculate* the effects of *continued action,*
" and to reflect, that *length of time* can convert ac-
" cidental into steady causes. Hence fragments of
" *rock*, from the *central chain*, are found to have
" travelled into distant vallies, *even where many*
" *inferior ridges intervene:* hence the *granite* of
" *Mont Blanc* is seen in the plains of *Lombardy*, or
" on the sides of *Jura;* and the ruins of the *Carpa-*
" *thian mountains* lie scattered over the shores of the
" *Baltic.*"

227. Mr. Playfair subjoins to this passage his note XVIII, in which he illustrates the text by the following explanation, p. 384.

" The *loose stones* found on the sides of hills, and
" the bottoms of vallies, when traced back to their
" *original place*, point out with *demonstrative evi-*
" *dence* the great changes which have happened since
" the commencement of their journey; and in par-
" ticular serve to show, that many vallies which now
" deeply intersect the surface, had not *begun to be*
" *cut out* when these *stones* were first detached from
" their native rocks. We know, for instance, that
" stones under the influence of such *forces* as we
" are

"are now considering, cannot have first *descended* "from one ridge, and then *ascended* on the side of "an opposite ridge. But the *granite* of *Mont Blanc* "has been found, as mentioned above, on the sides "of *Jura*, and even on the side of it farthest from "the *Alps*. Now, in the present state of the earth's "surface, between the *central change* of the *Alps*, "from which these *pieces of granite must have* "*come*, and the ridge of *Mont Jura*, besides many "smaller *vallies*, there is the great *valley* of the "*Rhone*, from the bottom of which, to the place "where they now lie, is a height of not less than "3000 feet. *Stones* could not, by any force that "we know of, be made to ascend over this height. "*We must therefore suppose*, that when they *tra*-"*velled* from *Mont Blanc* to *Jura*, this deep *valley* "did not exist, but that such a *uniform declivity*, "as water can run on with rapidity, extended *from* "*the one summit to the other*. This supposition "*accords well* with what has been already said con-"cerning the *recent* formation of the *Leman Lake*, "and of the present *valley* of the *Rhone*.

228. There are, I believe, few instances of gratuitous hypotheses more evident than this. Mr. Playfair does not mention that I had opposed the intervention of *vallies* and *chains of mountains* to such a transportation of *stones*; and that I had even assigned a *cause* why they were found scattered, not only on the sides of Jura, and in the plains of Lombardy, but likewise over immense tracts far remote from any mountain whatever. Yet, as if the preceding

ceding objection had originated with himself, and he was therefore under no necessity of answering it; as if he had refuted my system concerning the origin of these blocks, and established his own to the exclusion of all others; or at least as if, having compared the granite of the blocks on Jura with that of Mont Blanc, he had shown by evident characters that they could not have proceeded from any other mountain; he decides that "*we must suppose*" them to have migrated from thence. I shall not, however, confine myself to the above remark, nor to the observation that one reason of his trusting to this gratuitous hypothesis is that it "*accords well*" with the other, "concerning the recent formation of the Leman "Lake:" neither shall I content myself with asserting that I have seen what Mr. Playfair has not, the blocks on Jura, and the granite of Mont Blanc, and that the former might with as much reason be referred to the *Giant's mountains* in *Silesia*; I propose to trace back his mistakes to their origin, by following him through the whole of his exposition of this singular hypothesis, wherein will again be manifested the error of the supposed excavation of vallies, by running waters; on which every thing rests, as well in Dr. Hutton's system, as in several others equally ungrounded.

"We can derive," he proceeds, "in a matter of "this sort, but little aid from calculation; yet we "may discover by it, whether our hypothesis trans-"gresses materially against the laws of probability, "and is inconsistent with physical principles already "established

"established. The horizontal distance from *Mont* "*Jura* to the *granite* mountains, at the head of the "*Arve*, may be accounted fifty geographic miles. "Though we suppose *Mont Blanc*, and the rest of "those mountains, to have been originally *much* "*higher* than they are at present, the ridge of *Jura* "must have been so likewise; and though probably "not by an equal quantity, yet it is the fairest way "to suppose the difference of their height to have "been nearly the same in former ages that it is at "present, and it may therefore be taken at 10,000 "feet. The declivity of a plane from the top of "*Mont Jura* to the top of *Mont Blanc*, would "therefore be about one mile and three quarters in "fifty, or one foot in thirty; an inclination much "greater than is necessary for *water* to *run* on, even "with extreme rapidity, and more than sufficient to "enable a *river* or a *torrent* to carry with it *stones* "or fragments of rock, almost to any distance.

"Saussure, in relating the fact that pieces of *gra*-"*nite* are found among the high passes near the "summits of *Mont Jura*, alleges, that they are "only found in spots from which the *central chain* "of the *Alps* may be seen. But it should seem that "this coincidence is accidental, because, from what-"ever cause the transportation of these *blocks* has "proceeded, the form of the mountains, especially "of *Mont Jura*, must be too much changed to ad-"mit of the supposition, that the places of it from "which *Mont Blanc* is now visible, are the same "from which that mountain was visible when those

"*stones*

" *stones were transported* hither. It may be, however, that the passes which now exist in *Mont Jura* " are the remains of *vallies*, or beds of *torrents*, " which once flowed westward from the *Alps;* and " it is natural, that the fragments from the latter " *mountains* should be found in the neighbourhood " of those ancient *water-trucks*."

229. It would lead us too far to show, by an actual description of the spots themselves, that, if Mr. Playfair had been acquainted with them, he assuredly would not have hazarded such conjectures. He takes the same liberty with the passage of the *waters* and *blocks of granite* from the Alps, as a poet might allow himself in tracing that of Hannibal over their chain; and he does not reflect, that the millions of years required for such changes, would bury the *origin* of the human race in the night of time, and rank the final end of individuals among the dreams of imagination. But I hope to restrain this flight of his fancy in its first career, by returning with him to his *original eminence*.

" *Mont Blanc*," he had observed, in the passage quoted in § 223, " *supposing* that it is *unstratified*, " is *understood*, to consist of a mass that was melted " by subterraneous heat *under the strata*...already " *consolidated;*" which it has " *heaved up;* and has " been formed in the *concavity* so produced, as in a " mould......The covering of strata, thus raised up, " *may have been* burst asunder at the *summit*, where " the curvature and elevation were the greatest; but " the

"the *melted mass* underneath may have already ac-
"quired *solidity*, or may have been sustained by the
"beds of *schistus* incumbent on its sides. This
"*schistus*, forming the *exterior crust*, was imme-
"diately acted on by the causes of waste and decom-
"position, which have long since stripped the *gra-*
"*nite* of a great part of its covering, and are now
"exercising their power on the central mass......From
"the appearances of waste and degradation which
"these mountains exhibit, it is certain, that the
"*schistus* must have reached much *higher* than it
"does at present."

230. Thus, at the birth of our continents, the whole tract of country comprised between the chains of the *Alps* and of *Jura*, though the chains themselves, and the hills which separate these two different classes of mountains, are composed of strata of a great variety of genera and species, and though their masses have undergone such angular movements, as must have required considerable vacant *spaces*, was nevertheless occupied only by a *single mass*, of which the declivities were sufficiently *regular* to enable the torrents to bring down with them *blocks of granite*, from the *highest point* of the convexity to its boundaries; which were, on one side, the *Jura* of the present time, with the hills of *Franche Comté* and *Burgundy*, and, on the other, those of *Lombardy*. Such is the strange hypothesis of Mr. Playfair; an hypothesis, which must however have been also adopted, though silently, by all those who have maintained, and still continue to maintain,

maintain, the excavation of vallies by running waters; not considering the millions of years, during which, upon such a supposition, our continents must already have existed. But *time* cannot operate without the means of *action;* whence arise these questions: In such a situation of things, where must the *blocks of granite* have been, which were to be propelled? And next, where were the *torrents* that should have propelled them? These are questions, which had not occurred to such geologists; and which, being examined, will prove subversive of the whole theory.

231. The granite was "*covered by schistus;*" and Mr. Playfair does not venture to strip it of its "covering" to any considerable degree, lest it should have *flowed* out at the surface; for he dares not trust to the circumstance that it "*may have already ac-*" *quired solidity;*" because a mass of such an enormous size could not, at the time of its being "*heaved*" *up under the strata,*" and moulded in the "*con-*" *cavity,*" have been in a state different from *lava,* the mass of which is almost nothing, in comparison with that before us. Mr. Playfair, therefore, contents himself with observing that "the covering of" strata" (the schistus) "thus raised up, *may have*" *been* burst asunder at the summit, where the cur-" vature and elevation were the greatest." Here, then, the *granite* will make its appearance; but what cause can he point out capable of *fracturing* it subsequently into *blocks?* Facts would have here instructed him, if he had been acquainted with these

mountains;

mountains; and the place of facts might have been supplied by reflection. Blocks neither are nor can can be formed, excepting on those sides of the mountains which are already *abrupt* and *fissured:* there the water percolating through the crevices and interstices, and freezing during the winter, produces the effect of gunpowder; separating from the mass of the strata such parts as are fractured, and have no support without. Such is the process of degradation, where those abrupt sides have resulted from the catastrophes, by which the whole mass of solid strata was convulsed and shattered; as may be seen in all the sides of the mountains which are actually crumbling down, and even in quarries, where the size of the pieces obtained is limited, not so much by weight, as by the fissures of the strata. Now to show in what manner blocks are detached from mountains in their present state, is to prove that none could possibly have been separated from that mass of granite which Mr. Playfair supposes to have made its appearance, "*where the curvature and* "*elevation were the greatest;*" that is, toward the *middle* of the mass, the *convexity* of which could not be considerable, since he estimates the declivity on either side at only one foot in thirty.

232. No block of granite, therefore, could have been formed on that almost horizontal summit; but granting that there might be some, still where were the *torrents* to put them in motion? At such an elevation in the atmosphere, an elevation supposed by Mr. Playfair to have even been originally much *greater,*

greater, this convex mass would have become incrusted with *ice* of considerable thickness, beneath which nothing could have been put in motion: but besides, it is sufficiently evident that no torrents can ever exist on the highest parts of grounds. There remains, then, no assignable source of granite blocks, and Mr. Playfair's imagination is effectually checked in its first flight. It is therefore of no importance to consider the present or anterior connexion between the Alps and Jura; not a single fragment could either have been broken off or removed from this mass of granite, which had appeared at the highest point of the convexity, at the time when its declivity is supposed to have been uniform. Let us however admit the effects of detritus and waste, notwithstanding the certainty that the incrustation of ice would have prevented them. The "*schistus*," as Mr. Playfair justly observes, must have been acted on in the first place, as "covering" the granite; and the causes of decomposition must have exercised their power so much the more rapidly on the former, as it was inferior in hardness to the latter. The granite, indeed, would afterwards have been exposed to the action of the same causes; but not until a valley had already been produced at its foot, between itself and the schistus; and there, supposing it possible that blocks could have been formed before the granite had acquired abrupt and fractured sides, the migration of those fragments would have terminated.

233. This Mr. Playfair might have known, had he followed with strict attention the consequences of

his

his hypothesis. Above all, he would have seen how much at variance with it are his own remarks in § 348. " For the moving of *large masses of rock*," he says, " the most powerful engines without doubt " which nature employs are the *glaciers*, those lakes " or rivers of *ice* which are formed in the highest " vallies of the Alps, and other mountains of the " first order. These great masses are in perpetual " motion, undermined by the influx of heat from the " earth, and impelled down the declivities on which " they rest by their enormous weight, together with " that of the innumerable *fragments of rock* with " which they are loaded. These fragments they gra- " dually transport to their utmost boundaries, where " a formidable wall ascertains the magnitude, and " attests the force, of the great engine by which it " was erected. The immense quantity and size of " the *rocks* thus transported, have been remarked " with astonishment by every observer, (Voyages " aux Alpes, tom. II. § 854,) and explain sufficiently " how fragments of rock may be put in motion, even " where there is but *little declivity*, and where the " actual surface of the ground is considerably *uneven*. " In this manner, *before the vallies were cut out* in " the form they now are, and when the mountains " were still more *elevated*, huge *fragments of rock* " may have been carried to a great distance."

234. Mr. Playfair could not have retained such opinions, if he had read without prepossession the descriptions of M. de Saussure, to which he refers both here, and with respect to another object which will

will follow. Why are the glaciers loaded with "*fragments of rock?*" Because they exist in vallies the sides of which are continually *mouldering down*. Now how could a similar degradation have taken place in regard to that small quantity of granite, which, according to the theory, had penetrated the "schistus" at the summit only of the vast cupola? Mr. Playfair contents himself with referring to those parts of M. de Saussure's travels, from which he draws his conclusions; but it is highly important to read the descriptions themselves; I shall therefore introduce them; but that referred to in the above passage will be preceded by some others, to which we shall also be led by Mr. Playfair.

235. Perceiving, without doubt, that an immediate transportation of "*fragments*," such as he had just supposed, from the summit of the Alps to Jura, antecedently to the formation of vallies, could not apply to all the phenomena of blocks, he undertakes, § 353, to explain in what manner, after they are once detached from the mountains, they may continue to advance, without being urged by *impulsion* of any kind. He suggests, as a first cause of their migration, the *waste* of the soil under them, a waste more considerable on the side where there is any declivity; joined to the circumstance of the stone itself being *rounded* by exposure to the weather. Now it certainly might happen that some stones should thus make their way over an inclined surface; but when I shall come to describe their various situations, it will easily be seen that this account of their *migration*

tion cannot be applied to the vast accumulations of blocks, found on level grounds, very remote from any mountain.

236. Mr. Playfair next proceeds to suggest the idea of another accidental motion which detached masses may acquire, adducing, as an instance, what are called *rocking stones*, or in Cornwall, *Logan stones*. They are, in general, blocks of *granite*; and are sometimes found lying on hard calcareous strata, as upon Jura, but much more frequently on *granite*. This phenomenon occurs under various circumstances. In some places, the horizontal *strata* of granite (for, whatever Mr. Playfair's opinion may be, *granite* is *stratified*) have been considerably fissured, in a vertical direction, and have suffered partial subsidences, in the intervals of which remain a kind of pillars, horizontally divided, according to the strata of which they are composed, into rhomboids or parallelopipeds. When the granite has not been liable to be decomposed by atmospherical causes, these pillars have been preserved without much alteration; some instances of which I have seen on the ridges of *Hartz*. But when the granite has been subject to decomposition, all the angles have been worn away, and the causes of detritus and waste have exercised their effect so far in the separations of the strata, as to reduce the rhomboids into spheroids. I have seen a number of pillars in the latter state on the Giant's mountains, in *Silesia*, separately, in some places; while in others, several

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of them are disposed in a strait line, and, from a distance, present the appearance of so many high piles of gourds.

237. This phenomenon assumes a variety of aspects; thus, for instance, I have seen a number of these groupes, not rising above the surface more than the height of two or three strata, the masses of which, of considerable magnitude, are likewise reduced to spheroids, resting one on another, though they touch but in a small point, comparatively with their size. Now this is the case with the *rocking stones:* the surfaces in contact are of such small extent, and the uppermost mass is so nearly in an equilibrium, that, notwithstanding its magnitude, the strength of a single man applied to one of its sides is sufficient to change its centre of gravity; and though at first in a degree scarcely perceptible, yet the repetition of such impulses, at each return of the stone, produces at length a sensible oscillation, which afterwards, for a while, continues of itself.

238. Such is the phenomenon of which Mr. Playfair speaks; and after having remarked that these stones would fall from their pedestal, if "*a part* "*from one side*" were "*cut off*," he supposes that their equilibrium may in time be destroyed by an unequal decomposition of the parts of their circumference, which would occasion a progressive movement of the mass. He next proceeds to speak of other masses liable to split by resting on narrow bases, so as to lose their equilibrium, and be

 put

put in motion on an inclined plane. He then goes on, as follows, § *355*. " Very unexpected accidents " sometimes happen to disturb the rest of such frag- " ments of rock as have once migrated from their " own place. Saussure mentions a great mass of " *lapis ollaris*, (Voyages aux Alpes, tom. IV. § 1851,) " that lies detached on the side of a declivity in the " valley of *Urseren*, in the canton of *Uri*. The " people use this stone as a quarry, and are working " it away on the upper side, in consequence of which " it will probably be soon overset, and will roll to " the bottom of the valley."

239. When I shall come to M. de Saussure's description of the phenomenon in question, which is the only precise instance adduced by Mr. Playfair, after all this display of general means to put rocky fragments in motion, we shall see to what these resources amount. But I shall first observe how singular it is that his prepossessions should have led him to fix on this spot, in the course of a description of phenomena, the most evidently in opposition to his system respecting the formation of vallies by the action of the waters which we see running in them. I shall therefore for a moment postpone the consideration of the mass of *lapis ollaris*, in order to point out some features of the valley of Urseren, as they are described in M. de Saussure's work, beginning at § 1850. " The valley of *Urseren*, situated between " *l'Hôpital* and *Zum-dorf*, lies in a direction from " E. N. E. to W. S. W. In the space between these " two villages, and particularly in the bed of the

 " torrent

"torrent which runs through the valley, we found "that the rocks forming its basis had their planes "in nearly the same direction; so that this valley, "as indeed every other appearance evidently indi- "cates, must be considered as one of the longitudi- "nal vallies of the *Alpine chain*. In one or two "parts of its course, its strata are nearly horizontal; "but this seems to be the effect of accident; their "general position rather approaching to the ver- "tical. The mountains which border this valley on "the N. N. W. are very high; one of them, which "is directly opposite to Zumdorf, is called the *Mutz-* "*Berg*; another lying farther to the W. S. W. is the "*Spitz-Berg*.....its summit rises above all the other "mountains which surround *St. Gothard*, and is "constantly kept in view during the descent from "the *Hospice des Capucins* to the village of l'Hôpi- "tal: its sharp and indented projections are much "admired, being of singular force and boldness...... "But, although the lofty summits of the mountains "of this chain are granitic, yet their base, from *La* "*Fourche* to *Urner-loch*, is covered by strata of "calcareous stone or argillaceous schistus, which "*rest* against their foot. At the foot of the opposite "chain, which borders the valley to the S. S. E., "neither calcareous nor argillaceous beds occur; "but only *lapis ollaris*, which, inversely, is not found "on the other side."

240. I shall not at present dwell on this description, for the purpose of showing how positively it contradicts the hypothesis of the excavation of vallies

lies by running waters; while, at the same time, it points out the cause of their existence, from the angular motion produced in the strata, which has rendered them vertical;—because Mr. Playfair might still suppose the stream which flows through the valley of Urseren to have excavated it by pursuing the direction of some soft stratum. It is therefore farther necessary to take notice of the form of this valley, as described by M. de Saussure, ascending from the spot where lies the great mass of lapis ollaris, to which I shall afterwards return. "§ 1852. From "this point, I again ascended to l'Hôpital, and "thence proceeded to *Urseren*, or *Andermatt*, which "is the principal place in the valley, and three quar-"ters of a league to the east of l'Hôpital. The "elevation of the spot on which this village stands "is 726 toises. The bottom of the valley near An-"dermatt is so flat, that little doubt can be enter-"tained of its having been originally a lake. The "village is situated by the side of a torrent, which "issues from the confines of the *Grisons*, eastward "of the valley, and falls into the *Reuss*......§ 1853. "On following the borders of this torrent, near An-"dermatt, banks of schisti occur, which are some-"what remarkable.....their strata are of the most "perfect regularity, nearly *vertical*, running from "E. N. E. to W. S. W., and slightly dipping to the "S. S. E......§ 1854. As the source of the lower "*Rhine* is distant only three short leagues from An-"dermatt, and as in the road to it there is a view "of the pretty lake of *Oberalp*, I was desirous of "making this excursion, and I performed it on the

"28th

"28th of July, 1775. I first ascended a steep "eminence, which employed three quarters of an "hour, over schistous rocks of the same class with "those described in the preceding §, and situated "precisely in the same manner. I then came to a "valley covered with pastures: this valley, bearing "the name of *Oberalp*, is a continuation of that of "Urseren;.....it has the shape of a cradle; the mea-"dows rising on both sides to the foot of the steep "rocks which border it: these meadows are inter-"spersed with huts, but there are no habitations in "them for the winter. Two short hours brought me "from Andermatt to the banks of the lake of Ober-"alp. This lake is scarcely more than a quarter "of a league in length, but it occupies the whole "breadth of the valley, the bottom of which is in-"deed very narrow. Its waters, which are clear, "tranquil, and deep, fill the lower part of this green "cradle, and produce a singular effect, chiefly on "account of the masses of snow, which, on the side "not exposed to the rays of the sun, from time to "time descend close to the edge of the lake; two "small islands clothed with verdure contribute like-"wise to adorn it; and, at its eastern extremity, a "fine cascade falling from the summit of a very "elevated rock, serves to complete the beauty of "this romantic spot. The want of trees, however, "lessens the cheerfulness of the scene, for none grow "here, nor in the lower valley. A small wood of "larch only is seen, opposite to Andermatt; and "much care is bestowed on its preservation, as it

"protects

" protects the valley from the destructive effects of
" the *avalanches.*

" 1856.....As I could not easily approach the dif-
" ferent rivulets, forming by their junction what is
" called the source of the *Lower Rhine*, the point
" of which I thought it most interesting to ascertain
" the elevation, was the summit of the *Col*, where
" the waters separate, and run, some through the
" lake of Oberalp into that of *Lucerne*, and others
" immediately into the *Rhine*. I accordingly placed
" my barometer at the foot of a small cross, which
" stands on the highest part of this *Col*, and serves
" as a boundary between the country of the *Grisons*,
" and the valley of Urseren. On the 28th of July,
" 1775, at 20 minutes past 9, A. M. the barometer,
" corrected from the effect of heat on the mercury,
" stood at 22 p. 21; and the thermometer in the open
" air, at 11$\frac{1}{3}$. This observation, compared with
" that made at Geneva by M. DE LUC, jun. gives
" 1029 toises above the level of the sea.

" § 1857. The mountains, to the south of the
" eastern extremity of the lake of Oberalp, consist
" of *schisti* subject to degradation, the strata of which
" are not very distinct. It seems, however, that
" their position is nearly *vertical*, inclining in a slight
" degree to the N. N. W. towards the lake of Ober-
" alp, and that their planes are in a direction from
" E. N. E. to W. S. W. But the opposite moun-
" tains, to the N. E. of the lake, which constitute
" the base of the *Crispalt*, have a very decided
" structure.

" structure. They consist of *gneiss*, the grains of " which are more or less large, and which in some " places might be entitled to the appellation of " *veined granite*. The strata are *vertical*, or at " least nearly so, and they are constantly *parallel* to " the interior veins of the stone. Their planes are " in an exact direction from E. N. E. to W. S. W. " like the valley of Oberalp, and that of Urseren, " as far as La Fourche, above the source of the " *Rhone*. It is a singular circumstance to see two " such large rivers as the Rhine and the Rhone take " their rise at the two opposite extremities of a lon- " gitudinal valley, parallel to the direction of the " strata of the mountains which enclose it. For this " direction is the same throughout the whole extent " of the two sides of the valley, with the exception " of a few irregularities, not deserving attention."

241. Here then are two rivers, which, through a space of some extent, flow in opposite directions, both rising at an elevated point, common to two vallies which run in the same direction; and we shall presently see that this is one of the cases which Mr. Playfair, considering it in a general manner, brings as a proof that the running waters have hollowed out the vallies. For he conceives that he can here trace the effect of a force, which was " *nothing*, *or* " *evanescent*," at the most elevated point, and which increased on both sides, as the distance from that point increased; " the working of water collected " from the rains and the snows," being, he says, " the only cause we know of which is subject to this

" law,"

"law." But it may here already be perceived, as it will also evidently appear in the instance to which he applies the above remark, that "*this law*" itself does not exist; since we find that the valley of Oberalp, from its very origin, is bounded by high mountains, between which it assumes the form of a cradle; its declivities, covered with meadows, rising to the foot of the steep summits which command them, and a torrent precipitating itself from a lofty rock at the extremity of this valley, the bottom of which is occupied by a lake. Now could running waters, at their *minimum* of power, or even at their *maximum*, possibly form the *basin* of a lake? Assuredly not; on the contrary, when they have met with any such *cavity* on their course, they have had a tendency to fill it up with the debris of rocks, which at first they found in great abundance in their channels: this has been the case near Andermatt, where the bottom of the valley exhibits the appearance of a lake that has been filled up. Moreover, could a current, that instrument the "*repeated touches*" of which, according to Mr. Playfair, (Illustr. § 99,) may be recognized in our vallies, have cut out at different heights wide and horizontal vallies like those of Oberalp and Andermatt? Besides, what kind of *outlet* is there to this latter valley, described by M. de Saussure as "*cheerful and verdant?*" He inform us, in § 1859: "That this passage is so narrow as to admit only "the river; and that it has been necessary, for the "accommodation of travellers, to cut out a subter-"raneous passage, about 200 feet in length." So that the only idea suggested by the accurate descrip-

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tion of this valley, similar to so many others in the Alps, is that of the cause which we shall presently find assigned to them by M. de Saussure himself; namely, the erect position assumed by the strata during the angular movements of the great masses, which, after having suffered longitudinal fractures, rose up in chains, and were consequently separated by vallies.

242. In the course of these descriptions, I have passed over, as a phenomenon of very little importance, the mass of *lapis ollaris,* to which Mr. Playfair, though he had the descriptions before him, exclusively directed his attention; but I now return to that phenomenon, in order to transcribe what M. de Saussure says on the subject, in § 1851, referred to by Mr. Playfair. "In descending from St. Gothard, "I had met with fragments of *lapis ollaris;* I had "even observed pieces of it among the stones with "which the road is paved; but I had seen none in "their native spot. M. Réglin offered to show "me a quarry of it near the road which I had to "ascend in my return to l'Hôpital. When we were "about half way, he led me out of the high road, "and we went up some steep meadows, for the pur- "pose of seeing this quarry in the mountain, to S. S. "by E. of the valley. But I was greatly astonished, "when, instead of a quarry, I saw only a block of "this stone. The block, however, is of an enor- "mous size; it is upwards of 100 feet in length; "its height is considerable; and it is used as a quar- "ry. It is also *evident that it has not come there*

"*from*

"*from any great distance;* but it does not adhere to "the ground, nor will it even long remain in its "present place, if the people continue to work it "away on the upper side, where the stone is of a "superior quality; for it will lose its balance, and "roll to the bottom of the valley." This, without doubt, is very probable; but when the stone shall have reached the bottom of the valley, it will travel no farther; for nothing in the present state of the globe can effect the transportation of masses like this, when once they have reached horizontal planes, such as are found in all our vallies. Is this, then, any confirmation of the proposition advanced by Mr. Playfair in his § 112, "that length of time can con- "vert accidental into steady causes?"—a proposition supposed to be illustrated by the *Note;* which has necessitated me to bring forward the whole scenery, from which Mr. Playfair had selected a single point. It is much rather a confirmation of what I had explained in my Letters to Dr. Hutton; viz. that, on account of the state of disorder in which our continents were at their birth, a state exhibiting only vast ruins of strata with abrupt and precipitous sections, plains covered with rubbish, and everywhere many cavities; the tendency of the *causes,* "*acci-* "*dental*" as well as "*steady,*" which have since acted upon them, has been, and still continues to be, only to bring their several parts to a fixed and permanent state. And here a remarkable circumstance strikes us in M. de Saussure's description. After having asserted it to be "*evident*" that this stone did "*not come from any great distance,*" he adds

adds nothing to point out any place whence it might have proceeded. Now, if we consider his having begun with saying, that, "*in descending from St.* "*Gothard,*" he "*had met with fragments of lapis* "*ollaris, but*" that having "*seen none in their na-* "*tive spot,*" a friend had "*offered to show*" him "*a* "*quarry*" of the stone, and that he had been much "*astonished*" to find "*only a single block,*" we cannot doubt his having directed his attention to the rocks immediately above him. What follows, therefore, seems to be the reflection which naturally occurred to his mind, when he perceived in them nothing similar to this stone; "it cannot, however," he says, "have come there from any great distance." The probability of this remark, and the consequence resulting from it, will appear, when I shall proceed to adduce an example, similar to the above, on the subject of great masses of calcareous stone.

243. We shall again be led to descriptions given by M. de Saussure of very important phenomena, by the following passage from Mr. Playfair, § 357, which serves as a conclusion to the *Note* already cited, *on the transportation of stones,* as well as to the preceding *Notes, on Rivers and Lakes,* and *on Blocks and Gravel.*

"The system" he says, "which accounts for such "phenomena as have been considered in this and "some of the preceding notes, by the operation of "a great *deluge,* or *débacle* as it is called, has been "already mentioned. In Dr. Hutton's theory, no-
"thing

"thing whatever is ascribed to such accidental and "unknown causes; and, though their existence is "not absolutely denied, their effects, whatever they "may have been, are alleged to be entirely oblite- "rated, so that they can be referred to no other "class but that of mere possibilities. A minute "discussion, however, of the question, Whether "there are, on the surface of the earth, any effects "that require the interposition of an *extraordinary* "*cause*, would lead into a longer digression than is "suited to this place. I shall briefly state what "appear to be the principal objections to all such "explanations of the phenomena of geology."

244. I cannot proceed without offering some remarks on this passage, which is a very extraordinary one in two respects. And first, in speaking of a "*great deluge*," Mr. Playfair might seem to allude to my system; in which I think I have made it evidently appear that the *Deluge*, as recorded in the Book of Genesis, where we find it described as having resulted from the destruction of *ancient continents* which the sea overwhelmed, furnishes the only explanation of the birth of our continents, accordant with geological monuments; and that the *chronology* of that sacred Book is confirmed by all the effects of those causes which have acted upon the surface of our continents since their birth. If Mr. Playfair thought himself able to refute this system, his attempt would not have been a "*digression*;" at least I do not consider in such a light the discussion of his theory. Truth cannot be solidly established,

blished, without the previous removal of error. But neither here, nor in any other part of his work, does Mr. Playfair intend to advert to my theory respecting the *deluge;* the objections which he proceeds to state refer only to the expression *débacle* used by M. de Saussure; and this is the next very extraordinary part of this passage.

245. What I am now about to observe on this head has already been remarked, in a general manner, by the Rev. EDWARD NARES, in his *Bampton Lectures*, p. 315; namely, that there is no theory which contains more *extraordinary causes* than Dr. Hutton's; while Mr. Playfair, speaking of M. de Saussure's system, particularly considers his friend's theory, as having superseded "*extraordinary causes.*" M. de Saussure was the first who pointed out to us that the great chains of mountains consisted of strata, as well as mountains of less elevation, and hills; that these strata had been originally produced at the bottom of the sea, in a horizontal and continuous position, and that they had assumed the form of mountains, in consequence only of the angular movements of their masses, when separated by fractures. Dr. Hutton adopted this explanation, and referred these catastrophes of the strata to the era of the birth of our continents. This surely is no *ordinary cause;* nor is it become the less "*extraordinary,*" because Dr. Hutton has multiplied its effects, by ascribing to it a *repeated production of continents* upon our globe, at intervals of some *millions of years.*

246. In

246. In giving this account of the state in which mountains are found, M. de Saussure left it undecided, whether those portions of the dislocated strata, now the most elevated, had been lifted up, or whether those, now lowest, had subsided. He deferred the decision of this question, to the time when he should have completed the observations which he had in view, intending then to state the theory which should result from the whole, collectively considered; an intention which he expressed in each successive volume. An untimely death deprived physical science of the farther researches, and of the theory, of this eminent naturalist; but the subject before us does not depend upon the question which he left undecided. Whether the birth of our continents was owing to their elevation from the bottom of the ocean, as Dr. Hutton supposes, or was produced by the subsidence of other continents which the sea overflowed; in either case, the waters must have *retreated* from the surface of the new continents. Now it is this *retreat* of the sea which M. de Saussure has called the *débacle*. By whatever name Mr. Playfair may chuse to term it, he must of necessity admit its reality; and whatever may have been its effects, he cannot ascribe them to an *ordinary cause*. The whole of the above passage, therefore, which paves the way for the introduction of other objects, to which we shall presently come, is founded only on illusion.

247. M. de Saussure, undoubtedly, at first ascribed too much power to the *débacle*, not only, as we have already

already seen, for the transportation of fragments of stones to great distances from the Alps, but even for the excavation of some vallies. It is also true that, in his earliest remarks, he inclined too much to attribute the latter effect, to torrents formed by the rains and melting of the snow. In fine, though it is to him that we are indebted for the first correct notions respecting the formation of mountains, he was not always consistent in his expressions; for he sometimes speaks of successive *formations* of *mountains*, in passages where it is evidently to the different *strata* of those mountains that his remarks apply. But can it excite our wonder, that, when such great conceptions were first formed, even he, in whose mind they originated, should have intermixed with them some errors, or should occasionally have been betrayed into inaccuracy of expression? Yet himself pointing out, and that at a very early period, what might have led him to carry too far his assertions respecting the above supposed causes of vallies, he again recurs to his fundamental principle; for he says, in § 920, (the same to which we shall find Mr. Playfair refer his readers for the description of a valley:) " I do not pretend to affirm that the waste and " erosion produced by the rains, the torrents, and " the rivers, are the only cause of the formation of " vallies: the vertical position of the strata in the " mountains forces us to admit another, *of which I* " *shall speak elsewhere*. I wished only to make it " appear that the correspondence of the angles, when " occurring in vallies, does not prove that such val- " lies are the production of the sea." Such, in fact, is

is the sole object of M. de Saussure in this paragraph, in exhibiting the peculiar characters of different vallies, and that in opposition to the opinion of BOURGUET, which had been adopted by BUFFON. But he could not have forgotten the necessary consequences of the erect position of the strata, for in the section immediately preceding, (the 919th) beginning with the great scene displayed to his view from the summit of *Cramont*, he had shown that the formation of mountains must have proceeded from the disruption of the strata, and the angular movements of the separated masses. He had then concluded the section in the following words: "Such were the ideas "suggested to me by these new observations in 1774. "It will be seen, in the 4th volume, how far twelve "or thirteen years of continual observation and re- "flection upon the subject may have modified this "first germ of my conjectures." Now it is to this that he evidently alludes, when in the next section he says, as we have seen above, that this other "*cause of the formation of vallies* shall be treated "of *elsewhere*." But, unfortunately, his 4th and last volume has appeared without the promised theory.

248. Let us now attend to Mr. Playfair's comments upon these observations, in § 358, where he begins to state the objections which he had announced in the preceding section. "The general structure "of vallies among mountains, is highly unfavour- "able to the notion that they were produced by any "single great torrent, which swept over the surface

 "of

"of the earth. In some instances, vallies diverge, "as it were from a centre, in all directions. In "others, they originate from a ridge, and proceed "with equal depth and extent on both sides of it, "plainly indicating, that the force which produced "them was *nothing*, or *evanescent* at the summit of "that ridge, and increased on both sides, as the dis-"tance from the ridge increased. The working of "water collected from the rains and the snows, and "seeking its way from a higher to a lower level, "is the only cause we know of, which is subject to "this law."

249. It often happens that Mr. Playfair substitutes that which *should be*, according to his theory, for that which actually *is*; I have already had occasion to make this remark, and I shall here bring a new instance in confirmation of it. § 360. "Some "vallies," he says, "are so particularly constructed, "as to carry with them a still stronger refutation of "the existence of a *débacle*." This however can apply only to the *effects*; since the *débacle*, or retreat of the sea, is a *fact*. "These are the longitudinal val-"lies, which have the openings by which the water is "discharged, not at one extremity, but at the broad-"side. Such is that on the east side of *Mont Blanc*, "deeply excavated on the confines of the granite and "schistus rock, and extending parallel to the beds of "the latter, from the *Col de la Seigne* to the *Col de* "*Ferret*; its opening is nearly in the middle, from "which the *Dorea* issues, and takes its course through "a great valley, nearly at right angles to the chain "of the *Alps*, and to the valley just mentioned.....

"*Voyages*

"(*Voyages aux Alpes*, tom. II. § 920.) The force "which excavated this valley must have been *nothing* "at the two extreme points, viz. at the *Col de Seigne* "and the *Col de Ferret*, and must have increased "with the distance from each. It can have been "produced, therefore, only by the running of two "streams in opposite directions, on a surface that "was but slightly uneven, these streams at meeting "taking a new direction, nearly at right angles to "the former. A clearer proof could hardly be re-"quired than is afforded in this case, that what is "now a deep valley was formerly solid rock, which "the running of the waters has gradually worn away; "and that the waters, when they began to run, were "on a level as high, at least, as the tops of those "mountains by which the valley is bounded toward "the lower side."

250. It is probable that Mr. Playfair has entirely confined his attention to this § 920 of the *Voyages aux Alpes*, in which is given only a general idea of the situation of the above valley, and that, as has been remarked above, in the view of opposing it to the hypothesis of Bourguet, followed by Buffon; for it does not appear to me that he can have read the descriptions given by M. de Saussure of the extremities of that valley, where he would have seen how far "*the force* "*which produced it*" was from being "*nothing, or* "*evanescent,*" I shall begin with the description of the extremity on the side towards Col de Ferret, § 861. "The *Col de Ferret* is composed of foliated "grit and soft slate, the plates of which are vertical, "excepting where they rest against the primitive

 "mountains.

" mountains. Their direction is S. S. by W., like
" that of the valley......The descent is very rapid, and
" dangerous, even for mules, after rain, as the loose
" slate forms a very slippery soil. This declivity is,
" like the *Col*, composed of slate and foliated grit;
" but banks of calcareous stone of a slaty colour
" also occur, and this last stone constitutes exclu-
" sively the lowest part of the mountain on the side
" towards the central chain......§ 863. I employed an
" hour in descending to the first huts; they are
" called the huts of *Prés de Bar*, and, according
" to our observations, are 145 toises lower than the
" summit of the *Col*. Between these huts, and the
" fine glacier of *Mont Dolent*, there is a hill consi-
" derably higher than the foot of that glacier, from
" which it is separated by a deep valley.....§ 866. From
" the huts I descended to the bottom of the (great)
" valley, and there, for the purpose of observing the
" base of *Mont-Ru*, which separates the glacier of
" Mont Dolent from that of *Triolet*, I left the beaten
" track, and with some difficulty forded on my mule
" the torrent issuing from the foot of the former of
" these glaciers.......§ 871. Along the road there oc-
" curs, in the primitive chain, a succession of gla-
" ciers, which I shall not stop to describe. One of
" the most striking is a league distant from the base
" of Mont Ferret; it is formed by the combination
" of four or five others, which discharge their ice
" into the same basin. About a quarter of a league
" beyond this glacier, the valley widens; it assumes
" a more cheerful aspect, and is covered with huts,
" flocks, and pasture grounds. It here takes the name
" of

"of *Vallée d'Entrève*." Such is the description of this extremity of the valley. The Col, or Mont Ferret, is first rapidly descended; and at the distance of a league and a quarter from its foot, we already reach the bottom of this valley, which is from eight to ten leagues in length; having left behind the vast projections of the central chain which border it, forming distinct eminences, which separate the glaciers from each other. Are these the characters of an "*evanescent force?*" of a *force*, which being at the very origin of its existence, could scarcely have begun any work of excavation? Do we not here, on the contrary, trace evident marks of *subsidence?*

251. Let us now proceed to the other extremity of this valley, where we shall find characters still more striking of its real cause, and more absolutely exclusive of that, of which Mr. Playfair asserts that "*a clearer proof could hardly be required than is* "*afforded in this case.*" We will therefore recur to M. de Saussure's description of the *Col de la Seigne*, beginning in § 845. "We employed an hour and a "quarter in reaching the summit of the Col, from "the hamlet of the glacier. We there enjoyed, on "a remarkably clear and fine day, the beautiful "spectacle presented by this spot, which is elevated "1263 toises above the level of the sea. Imme-"diately beneath us was the *Allée blanche*, and, in "the same direction, the valley of *Ferret*, termi-"nated by the Col of that name. These two val-"lies, in reality, form only one, from eight to ten

" leagues in length, running from N. E. to S. W., " and bounded, at its two extremities, by the two " Cols, which are of the same nature, and nearly " of the same height. This long valley is bordered " on the left, or to the N. W., by the *chain of Mont* " *Blanc*, majestically towering above the high Needles " which surround it......The mountains which bor- " der this valley to the S. E. are less high, though " still of considerable elevation; they are *secondary*; " and for the most part *calcareous;* their summits, " nearly all of which terminate in a point, are ex- " tremely steep on the side next to the chain of " *Mont Blanc*, and incline even towards that ridge, " as if making an effort to reach it. From the bot- " tom of this valley, which is rounded in the form " of a cradle, arise, very near the spot where we " stood, two *pyramids*, extremely sharp-pointed, " and surpassing the Col in elevation. These two " pyramids are composed of a *calcareous stone* in- " termixed with *mica*.......§ 849.....The waters which " descend on that side of the *Seigne* discharge them- " selves into the *Po*, and from thence into the *Adri-* " *atic*......§ 850. Shortly after we began to descend, " we passed near some beds of *breccia* remarkable " for the flatness of its fragments. These beds form " with the horizon an angle of 51°, rising up against " the central chain; and they run, like the valley, " from N. E. to S. W......The breccia again occurs " towards the bottom of the descent, at the foot of " the calcareous pyramids already mentioned...... " § 851. The pyramid which is highest, and nearest " to the Col, appears also to be composed of this

breccia

"breccia; but the lowest is not a breccia; it is a "grey *calcareous stone*, traversed by veins of spar, "and occasionally of quartz. Nor is the *position* "of the strata the same in the two pyramids; in the "highest, they run from N. to S., while in the other "they run nearly from N. N. by E. to S. S. by W. "In other respects, their configuration is the same; "they consist of vast pyramidal plates, almost *ver-* "*tical*, applied one against another." These details may serve to show how conducive to error are all vague and indistinct descriptions of phenomena. The simple idea of a *valley*, the waters of which have no passage at its extremities, but only towards the middle of one of its sides, leaves the imagination at liberty to conjecture, that, at the extremities, the "*force*" which produced the valley might have been, as it were, in its infancy, and might afterwards have increased, as the distance from those points increased; a law, without doubt, applicable only to running waters which begin to flow at such extremities. But the foregoing description, by its details, entirely sets aside this idea, and refers us to a cause capable of having deepened the valley at its *extremities*, as well as towards the *middle*, with only accidental differences. Now such a cause is to be found in partial subsidences of masses of the strata, at the time when the remaining lateral masses underwent those *angular movements*, noticed by Mr. Playfair, which could not possibly have taken place, unless vacant spaces had existed between them. The effects resulting from these angular movements are ascertained by the position of the strata, in the ridges on either side of the valley.

152. The

252. The same cause manifests itself in another part of M. de Saussure's description, where he speaks of a lake in the same valley, very near to the Col de la Seigne: a circumstance extremely remarkable, and particularly noticed by the Reverend E. Nares, in his Bampton Lectures already cited, p. 317. "The *stream* from the *Col de la Seigne*" he observes, "passes through the *Lac de Combal*, which would "surely not have existed till this time, had that "stream been the vehicle of such a *detritus* as must "have been necessary to the formation of the valley." This is evident, and will become more so, by a consideration of the several phenomena which accompany this lake, as described by M. de Saussure.

"§ 852. And first we find," at the foot of the pyramids, "an extensive tract, partly covered with "*debris* and partly with meadows, at the extremity "of which are huts, bearing the name of the *Allée* "*blanche*. We leave these to the left, and pass on "to the foot of the vast glacier, also called that of "the *Allée blanche*. It is formed by the junction "of three glaciers, which discharge their ice into "the same basin. Two parallel ridges, composed "of earth and debris, contrast with the whiteness "of the ice, which in itself is pure, shining, and "deeply furrowed by fissures, through which ap-"pears, towards the bottom, the green hue, peculiar "to the Glaciers. Some of the rocks, the extreme "steepness of which prevents the accumulation of "the ice upon them, by thus breaking its continuity, "discover to our view the astonishing thickness of

"its

"its masses. The summit of the glacier is com-
"manded by a ridge of rock, which is itself crowned
"by a ledge of snow projecting towards the side on
"which we stood.....§ 853. Still keeping in sight this
"magnificent glacier, we descended into a plain ap-
"proaching to an oval form, at the extremity of
"which there is a *small lake*, called the *Lac de
"Combal*, or that of the *Allée blanche*......This lake
"and this little plain are commanded on the N. W.
"by a mountain, which separates the glacier of the
"Allée blanche from another called the *Ruize de
"Miage*......This *Glacier de Miage*, adjacent to the
"mountain on the N. E., is not seen from hence;
"for we are below it and it is concealed from our
"view by its *moraine*, or the accumulation of stony
"fragments and *debris* by which it is bordered and
"enclosed. This *moraine*, elevated above the sur-
"face from 100 to 150 feet, borders to the N. E. the
"small lake which we are coasting. The lake, the
"waters of which are of a blackish green, surrounded
"on every side by these heaps of ruins, and by de-
"clivities interspersed with larches stunted and half
"decayed, presents the most wild and dreary
"aspect."

253. We now come to the phenomenon, which had been adduced by Mr. Playfair in § 348, cited above, in proof that "before the vallies were cut
"out in the form they now are, and when the moun-
"tains were still more elevated, *huge fragments of
"rock may have been carried to a great distance.*"
Let us then follow this description, to which he refers

fers his readers in a note, and which we shall find in § 854 of M. de Saussure's Travels. "On quitting "the larches, we found ourselves at the extremity "of the lake; we crossed on a wooden bridge the "torrent which issues from it, and we followed its "course, and the *moraine* of the *Glacier de Miage*, "for the space of an hour. This route is wild and "desert in the extreme; the weight of the ice, con-"tinually pressing on the debris accumulated at the "foot of the glacier, causes them to fall into the "torrent, which is gradually undermining the moun-"tain on the opposite side......This route presents to "the lithologist the most interesting objects. He "finds there a great variety of rare and curious "stones, brought down by the *glacier* from the base "of *Mont Blanc*, which commands its left bank...... "Were it not known that these stones had been con-"veyed thither by a glacier, no circumstance could "have pointed it out. The debris are accumulated "in such vast quantities, as effectually to conceal "the ice, which is visible only at the bottom of some "furrows, scooped out by the waters that collect on "the surface. But, after having passed through "this narrow defile, we entered a cheerful valley, "covered with fertile meadows; and, on looking "back, we saw the entire base of the immense gla-"cier, over the rampart of which we had been tra-"velling."

254. I can readily conceive that Mr. Playfair, prepossessed in favour of his opinion, may have taken for a confirmation of it a part of the foregoing

passage; but the remainder might have undeceived him, if, having read this whole volume with attention, he had more particularly considered § 625; where, speaking of the *Glacier des Bois*, in the valley of *Chamouni*, situated on the opposite side of the central chain, M. de Saussure expresses himself as follows: "The blocks of stone, with which "the bottom of this glacier is loaded, lead to an "important reflection. When we consider the smallness of their number, and recollect that they are "deposited at this extremity of the glacier in proportion as the ice melts, we are astonished that "there is not a more considerable heap; and this "observation, as it agrees with many others which "I shall report in succession, may induce us to believe, with Mr. DE LUC, that the present state of "our continents is not so ancient as some philosophers have supposed it to be." M. de Saussure would have drawn a similar inference from the phenomena exhibited by the glaciers of the valley just described. *When we consider* that *glaciers* descending from the *chain of Mont Blanc*, border this valley to the N. W. from one extremity to the other, and that they "*deposit*" in it "*fragments of stone*, "*in proportion as the ice melts*," is it not evident that, if such a deposition and accumulation of stones had been continued for an *immense number of ages*, the bottom of the valley would at present be *covered with them*, and that no "*fertile meadows*," interspersed with huts and cattle, would now meet the eye? And, without entering at large upon the subject, since the *moraines* of the glaciers of the Allée blanche

blanche and of Miage, those accumulations of stones which border the lake of Combal, are advancing into it, as well as on the plains above and below it, and towards the torrent that flows out of it, what should have prevented them from having already filled up the lake, and covered all that space? In fact, these phenomena are of so striking a nature, that, were it not for the prepossession entertained by Mr. Playfair, they would assuredly have served to convince him that this valley, far from having been gradually *hollowed out* from a solid rock, is not so deep now, as it was at the birth of our continents: and, (returning to our immediate subject,) that none of the *fragments* brought down by the glaciers from the abrupt sides of the rocks, either have been or could have been transported out of the *valley*.

255. A clear idea of the importance of the phenomenon of the granite fragments found scattered in different countries may have been formed already, from the details to which I have been led in examining the several means employed by Mr. Playfair in support of Dr. Hutton's opinion on this subject. These geologists, more enlightened than the greater part of their predecessors, were the first to acknowledge, that, in supposing a transportation of these fragments over the surface of the soil, it must be considered that between many of the spots where they abound, and every granitic eminence, *chains of mountains* intervene, along the vallies of which they could not possibly pass; whence it became necessary to show in what manner such a transportation could have

have been effected, notwithstanding these obstacles. But it may have appeared, from the foregoing descriptions, that the means employed by Mr. Playfair for removing this difficulty are wholly inadmissible; and now, by adducing many other particulars of this remarkable phenomenon, I shall render manifest the impossibility of believing any kind of *transportation* of these masses along the surface of the ground, and the consequent necessity of referring them to an origin very different from that which had been thus assigned to them. And here I shall begin with observing that, when blocks of *granite* were supposed to proceed from *granitic* mountains, their *genus* only was considered; whereas it would have been proper to compare the respective *species.* In the heaps of blocks, for example, found in *Westphalia*, *Lower Saxony*, and *Pomerania*, there occurs an astonishing variety of *species*, not only of *granite*, but of *porphyry*, and other stones of the same class, entirely unknown in the *mountains of Germany*. This has induced some German mineralogists to suppose these blocks to have proceeded from the mountains of *Sweden*; the ice in the *Baltic* having served them as a vehicle for traversing that sea, of which, since then, the level has subsided. Others again, not assenting to such a cause, have imagined these blocks to be the *remains* of granite mountains formerly existing on those very spots, from the degradation of which is derived the sand found there in such abundance, intermixed in some places with great quantities of gravel of the same stones. But, if all the characters of this great phenomenon had been ascertained

tained with exactness, explanations so much at variance with each other would never have been hazarded.

256. It is not only at great distances from *granitic mountains* that the dissimilarity of the species of the granite in these mountains and in the blocks denotes that they must have had a different origin. In the vicinity of the *Giant's mountains*, and those of *Hartz*, I have seen granite blocks, of species very distinct from any found in those mountains. M. Patrin has observed the same phenomenon in *Daouria*, on the mountain *Odon Tchelonn*, where the granite of the blocks scattered over its declivity differs from that of the rocks which command it, whence, if they had proceeded from the mountain, they should have fallen. Similar cases occur in the Alps, as may be seen on comparing the stones found on the declivities with the overhanging rocks. I have adduced an instance of this, in § 167, on the authority of M. de Saussure, who mentions a number of *primitive stones* observable on the slope of a *calcareous mountain*, in the valley of *Cluse*. And in the chapter where he treats of the Lithology of the valley of *Chamouni*, after speaking of large accumulations of *lapis ollaris*, or *serpentine* stone, of which none is to be found in the rocks above, and describing other phenomena of the same kind, he makes, in § 717, the following remark, certainly very essential, inasmuch as it relates to the distinction to be made between the phenomena which have their cause in the operations now taking place in mountains, and those which were produced

produced at the time of the formation of the latter. " It is an observation," he says, " important to the " Theory of the Earth, that, in the upper parts of " vallies surrounded by high mountains, we find no " rounded stones, foreign from the nature of the par- " ticular valley in which we may chance to be; those " that we meet with are never any thing more than " the *debris* of the neighbouring mountains. In " plains, on the contrary, and at the outlets of val- " lies opening into plains, we find stones which seem " to have *fallen from the sky*, so widely different is " their nature from all that we see in the environs."

257. But the stones which are found disseminated over the surface of the land, and which belong to no mountain of the country, are not only *granite*, *porphyry*, and others of a similar kind; we see among them some of several other classes and species, and this is a very important circumstance of the phenomenon. We find scattered blocks of *granulated quartz*, of *laminated quartz*, of *grit*, of *waeken*, of *hornstone*, of *calcareous stone*, as well as gravels of all these classes, and of *flint*, on soils very remote from every mountain composed of these stones. Mr. Playfair himself adduces several examples of this phenomenon which occur in England, and in counties very familiar to me; but he does not connect them with many others, which lead to the exclusion of the origin ascribed to them in his work. The following is one of these examples, § 335. " The road " to *Exeter* from *Taunton Dean*, between the latter " and *Honiton*, passes over a large heath or down,

" con-

" considerably elevated above the plain of *Taunton*.
" The rock which is the base of this heath, as far
" as can be discovered is lime-stone, and over the
" *surface of it* large *flints* in the form of gravel, are
" very thickly spread. There is no higher ground in
" the neighbourhood from which this gravel can be
" supposed to have come, nor any stream that can
" have carried it; so that *no explanation* of it *re-*
" *mains*, but that it is formed of the *flints* contained
" in beds of limestone, which are now worn away.
" The *flints* on the heath are precisely *of the kind*
" *found in the limestone;* many of them are not
" much worn, and cannot have travelled far from the
" rock in which they were originally contained. It
" seems certain, therefore, that they are the *debris*
" of limestone strata, now entirely decomposed, that
" once lay above the strata which at present form the
" base of this elevated plain, and probably covered
" them to a considerable height. This explanation
" carries the greater probability with it, that any
" other way of accounting for the fact in question,
" as the travelling of the gravel from higher grounds,
" or the immersion of the surface under the sea, will
" imply changes in the face of the country, incom-
" parably greater than are here supposed. Our hy-
" pothesis seems to give the minimum of all the kinds
" of changes that can possibly account for the phe-
" nomenon."

258. In § 336, Mr. Playfair brings an instance of the dissemination of a gravel formed of *granulated quartz*. This gravel is found in great abundance in *Warwickshire*,

Warwickshire, Leicestershire, Nottinghamshire, and as far as the south of *Yorkshire,* although "*no rock* "*of the same sort,*" as Mr. Playfair himself says, "*is seen in its native place.* This enigma is ex- "plained," he continues, "when it is observed, that "the basis of the whole tract......is a red sandstone, "often containing in it a hard quartzy gravel, per- "fectly similar to that which has just been men- "tioned. From the dissolution of beds of this sand- "stone.....there can be no doubt that this gravel is "derived.....I have said," he resumes, "that a rock "capable of affording such gravel as this, is not to "be found in the tract of country just mentioned. "This, however, is not exactly true; for in *Wor-* "*cestershire,* between *Bromesgrove* and *Birmingham,* "about seven miles from the latter, a rock is found "consisting of indurated strata, greatly elevated, "and without doubt primitive, from the detritus of "which such gravel as we are now speaking of might "be produced. These strata seem to rise up from "under the secondary, where they are intersected "by the road; and, for as much as appears, are "not of great thickness, so that they cannot have "afforded the materials of this gravel directly, though "they may have done so indirectly, or through the "medium of the red sandstone; that is to say, a "primary rock of which they are the remains, may "have afforded materials for the gravel in the sand- "stone; and this sandstone may in its turn have af- "forded the materials of the present soil, and par- "ticularly the gravel contained in it."

259. After these explanations of particular phenomena, Mr. Playfair next enters into general speculations, to which I shall advert;—but not until I shall have first examined what he has said of those phenomena, in respect to which, thinking that he has considered them in all their circumstances sufficiently for the purpose of subsequent generalization, he cites the following important maxim of Bacon: "That "the explanation of a phenomenon should not be "sought for from the study of that phenomenon alone, "but from the comparison of it with others." It were to be wished that he had really availed himself of such means of arriving at certainty, before he had given those explanations of the above phenomena, which I shall now proceed to examine.

260. In the first example which he adduces, he dwells only on one of the phenomena of the particular kind which he cites, namely, the gravel formed of flints, which is found on the road from Taunton to Honiton, and which in general covers the *Blackdown* hills; because he imagines that this gravel may be attributed to the destruction of calcareous strata, of the same kind as some which are here and there discovered among those hills. But he has not observed them so attentively as I have, and it will be seen in my travels both that his explanation is absolutely excluded by facts, and that no other cause can be assigned to this, and to many other similar phenomena, than operations which took place in the ancient sea.

261. With regard to the second example, where Mr. Playfair speaks of *granulated quartz*, he adverts only to the *gravel* of that class disseminated over some counties; and he ascribes its origin to *strata* of a similar kind, which, in the first place, produced the *red sandstone* intermixed with *quartzy gravel*, the sandstone itself being afterwards decomposed. Upon which I shall begin with remarking that, considering the operations now going on upon our continents, it is impossible to suppose the existence of those which he here accumulates on each other; namely, the formation of this gravel by the destruction of strata of its own species, its dispersion over so great an extent of country, its incorporation in real strata of sandstone, and finally the decomposition of the latter. I have visited that part of England since I have been acquainted with his work; and it will be seen in my travels how many circumstances relative to this very phenomenon had escaped his observation. I shall here add only one other phenomenon, which is also common to *Warwickshire*, *Staffordshire*, *Shropshire*, and *Lancashire;* I mean the great quantity of *granite* blocks found in these counties, besides gravel and some blocks of *granulated quartz*; although the only granitic eminence in the vicinity of that extensive tract is *Mount Sorrel* in *Leicestershire*.

262. But, with respect to granulated quartz itself, Mr. Playfair appears to be unacquainted with a phenomenon, in comparison with which all this gravel must sink into insignificance. In *Berkshire*, *Buckinghamshire*, *Wiltshire*, and *Dorsetshire*, where

 there

there is only sand and chalk, vast quantities of large blocks of granulated quartz are found in the sand, which blocks the inhabitants break into cubes, for the purposes of paving and building. *Windsor Castle*, for instance, and its terrace, are constructed with this stone, which was probably taken from the forest or the neighbouring heaths, where, nevertheless, a great quantity of it still remains. The fragments of granulated quartz on *Marlborough* downs are more generally known; and to give an idea of the magnitude of some of these blocks, it will be sufficient to state that the stupendous druidical monument on *Salisbury* plain, called *Stonehenge*, is formed of them. To assert these primitive stones to be the remains of strata of their particular species, by which the chalk was formerly covered, would be framing an hypothesis similar to that, which derives the granite blocks of *Brandenburg*, *Mecklenburgh*, and *Pomerania*, from granitic mountains anciently occupying these tracts; whence also the sand, which now covers them to such a depth, is supposed to have originated. We must then have recourse to a far different cause, common to all these phenomena.

263. Proceeding afterwards to generalize his particular remarks, Mr. Playfair expresses himself as follows, § 342: "It is a fact very generally observed, that where the vallies among primitive "mountains open into large plains, the gravel of "those plains consists of stones, evidently derived "from the mountains. The nearer that any spot is "to the mountains, the larger are the gravel stones, "and the less rounded is their figure; and, as the "distance

" distance increases, this gravel, which often forms " a stratum nearly level, is covered with a thicker " bed of earth or vegetable soil. This progression " has particularly been observed in the vallies of " *Piedmont* and the plains of *Lombardy*, where a " bed of gravel forms the basis of the soil, from the " foot of the *Alps* to the shores of the *Adriatic*. We " may collect from GUETTARD, that a similar gra- " dation is found in the gravel and earth which cover " the great plain of *Poland*, from Mount *Krapack* " to the *Baltic*. The reason of this gradation is " evident; the farther the stones have travelled, and " the more rubbing they have endured, the smaller " they grow, the more regular is the figure they as- " sume, and the greater the quantity of that finer " detritus which constitutes the soil. The washing " of the rains and rivers is here obvious; and each " of the three quantities just mentioned, if not di- " rectly proportional to the distance which the stones " have migrated from their native place, may be said, " in the language of geometry, to be at least pro- " portional to a certain function of that distance.

" The immense quantity of *cailloux roulés*, or " rounded gravel, collected in the immediate vicini- " ty of mountainous tracts, has led some geologists " to suppose the existence of ancient currents, which " descended from the mountains, in a quantity, and " with a *momentum*, of which there is no example " in the present state of the world. Thus Saussure " imagines, that the hill of *Supergue*, near *Turin*, " which is formed of gravel, can only be explained " by supposing such currents as are just mentioned,

" or

"or what he terms a *débacle*, to have taken place at "some former period. If however, we ascribe to "the mountains a magnitude and elevation vastly "greater than that which they now possess; if we re-"gard the vallies between them as cut out by the "rivers and torrents from an immense rampart of "solid rock, neither materials sufficiently great, nor "agents sufficiently powerful, will appear to be want-"ing, for collecting bodies of gravel and other loose "materials, equal to any that are found on the sur-"face of the earth. The necessity of introducing "a *débacle*, or any other unknown agent, to account "for the transportation of fossils, seems to arise from "under-rating the effects of action long continued, and "not limited by such short periods as circumscribe "the works, and even the observations, of men."

264. Mr. Playfair himself has speculated much more than he has observed; and, in regard to the observations of others, he dwells only on those which seem favourable to his system. If his theory were true, appearances ought indeed to be such as he states them at the beginning of this passage; but nothing can be more at variance than that description is with facts. With respect to the circumstance which he adduces on the authority of M. Guettard, I shall only say, that I have observed the phenomenon of the stones which lie scattered on the shores of the Baltic, and that the description which I shall give of them in my travels will show the error of supposing them to have been transported thither by the impulsion of any such cause as he assigns. But I shall dwell more particularly on what he advances from M. de

M. de Saussure, of whose testimony he avails himself when it appears favourable to his own hypothesis, as in the present instance of the state of the plains watered by the *Po*, while he contradicts him upon other facts, although unacquainted with that scene. If vallies have owed their formation to running waters, and if, as he supposes, their materials have already in part been conveyed to the sea, appearances must indeed be such as he states them, when he says that " where the vallies among primi-" tive mountains open into large plains, the gravel " of those plains consists of stones, evidently de-" rived from the mountains." But he has not here attended to M. de Saussure's remark cited above, that, on the contrary, " in plains, and at the out-" lets of vallies opening into plains, we find rounded " stones, and fragments of rock, which seem to have " *fallen from the sky*, so widely different is their " nature from all that we see in the environs." Can any thing be in more direct opposition to what Mr. Playfair asserts to have been generally observed? The fact, however, as stated by M. de Saussure, is well ascertained, and extremely common; and he brings several examples of this phenomenon in his travels. I do not agree with him in opinion respecting some of the effects which he ascribes to the *débacle*, such as the blocks of granite found on Jura, and on the hill of *Supergue*: but we shall presently see that he does not imagine the production of that hill itself to have been one of those effects, as Mr. Playfair asserts; and besides, this *débacle*, to which several phenomena connected with the transportation of loose materials may doubtless be attributed, is

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an event which unquestionably happened, and which Mr. Playfair himself must admit. For, as I have already observed, M. de Saussure understands only by that event the *retreat* of the *sea* from our continents at the period of their *birth*; a retreat which must have been sudden and violent, whether these continents were *raised*, as Mr. Playfair thinks, or other continents *subsided*.

265. Mr. Playfair, then, has substituted what *ought to be*, conformably to his hypothesis, for what really *is*, and has set aside such facts as demonstrate the actual state of things to be extremely different; and this with regard not only to those brought forward by M. de Saussure, whom he cites, but also to those mentioned in my works, of which he never speaks. In my second Letter to Dr. Hutton, I gave a statement of facts, no less certain than they are general; they will not be effaced by the silence with which he passed them over, but will ultimately prevail over the mere suggestions of the imagination. Most of the materials, set at any time in motion by running waters among mountains, still remain there, and no part of them is conveyed by rivers beyond their boundaries, but through such spaces as possess a sufficient degree of declivity. For, as soon as these rivers reach the plains, their beds are found to contain no other gravel, than such as belonged to those plains at the period of the birth of our continents. This is evident; inasmuch as the gravel changes in the beds of rivers, whenever any difference occurs in that of the soils through which those rivers run; and this is the case with regard to the soil of the neighbouring

neighbouring hills, as well as to that of the plain. Now Mr. Playfair ought to have noticed and answered this fact, before he considered himself as authorised to say, that all the materials found on the plains were *evidently derived from the mountains*. It is not surprising that the Po, in its short passage from the mountains to the Adriatic, should have no stones on its bed different from those belonging to the mountains; for such was the soil found by that river, when first it began to flow there. The plain of *Piedmont* owes its origin to a very considerable subsidence of the surface; and its general aspect cannot be more accurately described than in M. de Saussure's words, § 1305, in the very chapter, whence Mr. Playfair cites § 1304. Having taken his station upon the hill of Supergue, (to which we shall presently come,) he says: "Beneath our feet lies "the plain of Piedmont, and we see the Alpine "ridge terminating this plain, and towering above "it, as a lofty wall rises above a garden.....This "plain, watered by the *Po*, and the two *Dorias*, "presents the finest and most luxuriant scenery..... "The mountain, on the summit of which stands the "church of *Supergue*, wears an aspect equally interesting and diversified; it forms a part of an extensive range of small mountains, all connected "with each other, which collectively bear the name "of the Hills of *Mont-Ferrat*.....The Hills of *Astesan* compose another similar assemblage; they run "at first in a direction nearly parallel with those of "*Mont-Ferrat*, then uniting with them on the north "side, they thus inclose a fine plain in the form of "a horse-shoe, open towards the south." All these hills,

hills, composed of sand and secondary strata, are the work of the sea, subsequently to the subsidence which left the future *Alps* so abrupt on one side, and the *Apennines* on the other. I mean to resume the consideration of this interesting country, but not until I shall have completed my remarks on the general object, since Mr. Playfair considers himself as having generalized all the phenomena which engage our present attention.

266. He had said in the passage last cited, "If "we ascribe to the mountains a magnitude and elevation vastly greater than that which they now "possess; if we regard the vallies between them as "cut out by the rivers and torrents from an immense "rampart of solid rock, neither materials sufficiently "great, nor agents sufficiently powerful, will appear "to be wanting, for collecting bodies of gravel and "other loose materials, equal to any that are found "on the surface of the earth." In this manner did he think it possible to account for the blocks of granite found scattered on Jura, as well as for those disseminated over the plains of Lombardy. We have seen in how many respects this explanation is erroneous with regard to Jura; it would be found equally unsatisfactory in its application to Lombardy; since, after what we learn from M. de Saussure of the Alps on that side, it is impossible to suppose that there can ever have been an uniform declivity, stretching from the central point as far as those plains. But let us examine the object under a more general point of view.

267. If

267. If the powerful agents supposed by Mr. Playfair were real, they should furnish some satisfactory account of all the *blocks of granite* dispersed over the surface of the earth; and he ought especially to be authorised by facts in his assertion, that "the "*nearer* any spot is to the mountains, the *larger* "are the gravel stones, and the *less rounded* is their "figure;" that, "as the *distance increases*, this gra-"vel.....is covered with a thicker *bed of earth or ve-"getable soil*," and that, "if this is not directly *pro-"portional to the distance* which the stones have mi-"grated from their native place, it may be said to "be at least proportional to a certain *function* of "that distance." Such is the consequence of bestowing no kind of attention on facts irreconcileable with preconceived notions. In my first geological work, already known to Mr. Playfair, and in all my subsequent writings, I have mentioned the blocks of granite, and other primitive stones, which are disseminated over various tracts in *Germany*. According to Mr. Playfair, therefore, we ought to be able to trace these back to the mountains, and perceive them increase in size, as we should draw nearer to the sources whence they were derived. But I will here state the character of that great phenomenon, as it will appear in my Travels. Blocks of granite, porphyry, and other stones of different kinds, are found accumulated in spots, where, according to M. de Saussure's expression, they "*seem to have fallen "from the sky*;" for they are surrounded by tracts in which few or none occur; and the district where I have observed the largest, of about twenty feet in diameter, is that which is the most remote from every

every granitic mountain;—I mean the country of *Holstein*. Can we expect the surface of the soil to discover to us any vestiges of the route pursued by these stones, when their heaps form as it were islands in tracts of land where none appear? Had Mr. Playfair at all noticed the foregoing incontrovertible fact, he could not possibly have maintained his opinion; he would have been aware that the hypothesis of long continued action is a mere play of the imagination when no traces of such action are discernible in the phenomena to be explained.

268. These striking and very general phenomena ought, I say, to silence all fanciful conjectures: I shall proceed, however, to mention two others, of the kind called by Bacon *instantiæ divortii*, which directly tend to overthrow every erroneous hypothesis. The first of these again relates to blocks of granite. I observed it in a chain of mountains in Hesse, the highest part of which bears the name of *Mesner*, or *Weisner*. There a bed of fossile peat occurs of considerable thickness, containing a great number of trunks, branches, and roots of trees, in the same manner as recent peat, but covered by a great depth of stony strata, of the basaltic kind, and resting on a bed of limestone. All these strata have been subjected to violent catastrophes, and have suffered disruptions and partial subsidences, in which the bed of peat has participated. Thus a large valley has been formed, in one of the sides of which the bed of peat discovers its section at a certain height, while the strata descend on the side opposite. This is one of the phenomena, which, like that

that of beds of coal, I have ascribed to islands formed in consequence of the catastrophes happening at the bottom of the ancient sea; islands, which, after having been covered with peat, did themselves subside below the level of the waters, where fresh strata were deposited on the peat, the greater part of which was mineralized into coal; after which new catastrophes fractured and dislocated all the strata. Peat beds of the above kind are worked in several places for the purpose of taking out the wood, the peat itself not being so good a fuel, and are entered upon where the section shows itself at the surface: but at Mount Mesner it has been found necessary to penetrate through the inclined limestone strata, in order to arrive more easily at the peat. When I had entered a considerable way into one of the galleries, several large *blocks of granite* were shown to me, which had been met with at the bottom of the peat, and left on the spot. Now can it be supposed that blocks, thus found under a covering of stony strata which had undergone the same catastrophes as all the others had suffered, were ever detached from granite mountains, to travel over the surface of our continents?

269. The other phenomenon is so much the more remarkable, as fragments of granite are not its object. My late friend Baron DE REDEN, Inspector general of the mines of *Hartz*, conducted me to the place which I am about to describe, in order that I might observe this phenomenon, well known to him, as he had an estate in the same valley, belonging to the territory of *Hildesheim*. The mountains,

which

which border this valley, consist of a calcareous stone; its strata are thin, and but slightly indurated, presenting their section towards the valley, the breadth of which is considerable. He showed me, on the declivity of one of the sides of the valley, a range of large insulated rocks, rising above the woods, and we went to examine some of them more closely. These are also composed of *limestone*, but its strata are harder, and of greater thickness; they contain marine bodies, but of a different kind from those in the strata of the mountain. These rocks are used as quarries; and some of them having been entirely worked away, these immense masses have been found to rest only upon the debris of the strata of the mountain, without adhering to it. In fact, throughout all the country no regular stratum of this stone has been found in its native place. Baron de Reden was acquainted with my theory relative to blocks of granite and other primordial stones, as published in my first geological work, and he had brought me to this valley for the express purpose of making me observe that the explanation which I had given was no less applicable to the *secondary* than to the primary strata; since it was evident that these enormous masses of calcareous stone could have proceeded only from below the surface, at the period of that catastrophe by which the valley was produced. We afterwards made an extensive circuit in pursuit of blocks, in the course of which we had the opportunity of observing many interesting phenomena, which will be described in my travels.

It

It was in this tour that we visited together Mount Mesner*.

270. This class of phenomena has not as yet been considered with sufficient attention, and for the reason already mentioned; namely, that in comparing the scattered blocks with the strata of mountains, the *genera* only, and not the *species*, have been noticed. The calcareous masses just described are as foreign from the nature of the mountains on the declivity of which they occur, as are the fragments of granite on the declivity of *Odon-Tchelonn* from the steep rocks on its summit, and the masses of *lapis ollaris* in the valley of Chamouni from the rocks above. When scattered masses shall be observed under this particular point of view, instances will multiply of this circumstance in the general phenomenon, and the erroneous opinion which has prevailed concerning the origin of these masses will be ultimately relinquished. I shall add but one more example similar to the last of these phenomena.

271. One of my nephews, whom I have had already occasion to mention, has applied very much to geological inquiries; and as I had communicated to him an account of the circumstances accompanying this valley in Hildesheim, he wrote to me from *Ireland*, in 1796, that he had observed in the vicinity of *Dublin* a phenomenon of the same

* This journey is one of those which will be published only in the French edition.

kind. In constructing the aqueduct of *Kildare*, it had been found necessary, at the distance of eight Irish miles on the west of Dublin, to form a communication between two hills, by an embankment about an hundred feet in height, the materials of which had been taken from one of those hills. These materials consist only of debris, among which there are rounded masses, from two to six feet in diameter, of a calcareous stone very different from that of the strata of the hill, which are thin, and but slightly indurated. The stone of which the blocks are composed, belongs to strata of greater thickness; it is very hard, and of a bluish colour. These blocks are broken for the purpose of making lime, which is much better than that obtained from the strata of the hill. Some rocks of the same stone appear at the surface in different parts of that district. There are three on the east of Dublin; one near *Rahenny*, four miles from that city, another two miles farther, and the third at the distance of seven miles. They are worked for the lime, and the labourers told my nephew that there were no others in the country; he saw one, however, at *Slane*, 24 miles to the north of Dublin. The thin external strata which prevail in those parts dip into the soil with an angle of from 30 to 40 degrees, being truncated at the surface in so strait a plane, as to excite my nephew's surprise: this appearance is observable on several hills, but generally with less regularity than in the present instance. Here then are evident marks of the catastrophes, which brought up to the surface these ruinous masses of the lowest

strata,

strata, the fragments of which were thus dispersed. Ireland furnishes another phenomenon of the same class. In the northern part of the County of *Clare*, to the westward of the island, is a plain thickly interspersed with calcareous blocks, some of which are of an immense size; they contain marine bodies, principally madrepores. This plain is terminated by a range of calcareous hills, deeply intersected, and presenting towards it their abrupt fronts divided into different platforms. Here then are evident vestiges of a depression, during which fragments of the subsiding strata were thrown upwards.

272. Another consideration will bring us still nearer to the only possible cause of all these phenomena, a cause to which they are referable under all their various forms: I had already developed it, on account of its great importance to the history of the earth, in my first geological work; but Mr. Playfair has passed it unnoticed like the rest. It appears that the catastrophes, which reduced our strata to heaps of ruins, must have acted for a longer space of time upon the *hills* and *plains*, in proportion as they are more depressed than the *mountainous tracts*; since on those depressed parts were accumulated in the greatest number the secondary strata of various kinds, themselves broken and dislocated, and covering the ruins of the primitive; and there did repetitions of catastrophes continue to take place, until the final retreat of the sea from our continents, involving in these convulsions those strata of sand,

 which

which are thus the most covered with blocks of different kinds of stone, and in which sea shells are found in such a state of preservation, as positively forbids us to suppose many ages to have elapsed, since they were abandoned by the waters of the ocean. Another passage of Mr. Playfair's work will now lead us back to a tract of country already mentioned, which is very remarkable in this respect; and which I have repeatedly visited; I mean *Piedmont* and *Lombardy:* it consists, in great measure, of *sand-hills*, the strata of which participated in the latest of the catastrophes undergone by the stony strata. In these sand-hills are found marine shells, so well preserved, that they seem to have been recently taken from the sea, having retained their gloss and natural colour, much better than those which we see upon the coasts. Among others, I met with a large oyster, the hinge of which became soft when immersed in water; and a large *cama* filled with sand, in which were several small shells of the same kind, but containing no sand, probably because they had closed at the moment of this sudden catastrophe; and within these last I found the animal still in a soft state. In another sand hill, some of the beds of which were hard and laminated, I found between the laminæ leaves of different trees, such as oak, alder, and willow, which had contracted only a brown tint; and they were intermixed with sea-shells. In the same tract of country, fossil stags' horns have been discovered. These circumstances prove one of the islands of the ancient sea to have existed there, which, after having been stocked with

 animals

animals and plants, subsided below the level of the water. To this interesting theatre of revolutions, which forms an extensive tract between the *Alps* and the *Apennines*, consisting of hills and plains, and stretching as far as the *Adriatic*, we shall be brought back by the following passage of Mr. Playfair, § 344.

"The supply of gravel and *cailloux roulés*, for "the plains extended at the feet of primitive moun- "tains, is doubtless in many cases much increased "by the pudding-stone, interposed between the se- "condary and the primary strata. The beds of "pudding-stone contain gravel already formed on "the shores of continents, that ceased to exist be- "fore the present were produced; and the cement "of this gravel, yielding easily to the weather, "allows the stones included in it to be washed "down by the torrents, and scattered over the "plains. I know not if the hill of Superga, "above mentioned, is not in reality a mass of the "*pudding-stone* which forms the border of the Alps, "and of which the materials have suffered no trans- "portation since the time of their last consoli- "dation. This at least is certain, that Saussure, "notwithstanding his accuracy, has sometimes con- "founded the *loose gravel* on the surface with that "which is *consolidated into rock*; an inaccuracy "which is to be charged, as I have elsewhere ob- "served, rather against his system than himself."

 274. What

273. What is here said of M. de Saussure is really more applicable to Mr. Playfair, whose own system has prevented him from reading the work of that able naturalist with sufficient attention. We have already seen M. de Saussure's description of the hill of Supergue, which is separated from the Alps by a plain, and forms the first range of a vast assemblage of hills. Having measured its height with the barometer at the most elevated point, where stands the church of Supergue, I found it to be 1232 feet above the city of *Turin*, which is situated at its foot. M. de Saussure informs us, § 1304, that " the declivity of the mountain, and " its top, are covered with gravel and other stones, " and even with *blocks*, of *granite*, *porphyry*, and " especially *serpentine*, the fragments of which last " are found in greater abundance and variety than " those of the others." These phenomena he attributed to the *débacle*, or retreat of the sea from our continents; and they cannot certainly by any means be referred to the agency of the causes supposed by Mr. Playfair: M. de Saussure had a much more powerful agent in his *débacle*, although the present case is not one to which it can be applied: he had not had opportunities of seeing so many hills as I have visited, far remote from every mountain, and covered with a much greater quantity of blocks of granite and porphyry, than that of Supergue.

274. But the description given by so accurate an observer as M. de Saussure of the interior parts of the hill, should have removed all possibility of sup-

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posing it a mass of pudding stone; and from his account Mr. Playfair might easily have collected the nature of the revolutions undergone by that part of the bed of the ancient sea, subsequently to the formation of the secondary on the ruins of the primitive strata; the shattered and dislocated masses of these latter, which then remained elevated above the rest, now constituting the Alps and Apennines. The following is M. de Saussure's description of the hill of Supergue, § 1304 E. "The body of the "mountain is composed of alternate strata of sand, "clay, and argillaceous limestone. The strata of "this stone are more solid towards the bottom than "at the summit of the mountain. All these strata, "or at least the greater part of them, dip to the "north; their inclination varies; in several places "they form an angle of 45°; but frequently they "are found less inclined." These characters should have pointed out to Mr. Playfair the effects of a catastrophe, which had shaken the whole mass of the strata from its lowest part. He has himself remarked, in the passage which has been already cited in § 173, that "*strata much inclined must* "*terminate abruptly where they come up to the sur-* "*face;*" a circumstance, he says, denoting "*the* "*violence with which they have been displaced,*" and the "*angular movements*" of the separated masses. Now this is exactly what has happened here; the difference of those angular movements in the masses having occasioned the different inclinations of the strata, and thus produced a succession of eminences, and in many places of abrupt sides,

discovering

discovering to us in their sections the various kinds, and several degrees of inclination, of strata which would have remained unknown, had they not by such means been brought to the surface. In consequence also of the same catastrophes, fragments of the lowest strata have been thrown up externally, as I shall presently explain. But let us first attend to the other characters of this hill.

275. Describing the contents of these strata, M. de Saussure says: "The sand beds of the upper "part of the mountain contain a great quantity of "rounded fragments, but the calcareous strata of "the lower part contain none; at least I could not "meet with any.......But I found the remains of "marine shells, particularly of the bivalve kind. "I likewise observed shells of the same class in "some strata of this mountain, consisting of a "yellow agglutinated sand; and among others I "found part of an oyster, which had retained all "the *freshness* and *brilliancy* of mother of pearl." The particular tendency of this last remark will soon appear. With regard to the strata of this hill, M. de Saussure, not having visited it so frequently as my brother and myself, had not observed a stratum, which evidently proves that its gravel had already existed at the bottom of the sea. That stratum, which is much inclined, contains a great quantity of shells, univalves as well as bivalves, and consists only of small gravel and detritus of the *serpentine stone*. Now we found in it an oyster, adhering to one of the rounded pieces of that stone, in the same manner

manner as they adhere to other bodies; and two shells of the species named in French *Fripière*, and in Latin *Trochus onustus*, because they load themselves with whatever small bodies they find in their way, generally with pieces of broken shells; but to those which we here met with were attached small rounded fragments of *serpentine*. These animals must therefore have existed on a *gravelly* bottom, which afterwards suffered the catastrophes so deeply impressed on the hill itself. In another hill, remote from this last, and containing gravel of different primitive stones, I also found some breccias, in which, as well as in the sand that surrounded them, marine shells were imbedded. In one of these masses I found a volute, the lines and colours of which are extremely well preserved.

276. These facts are detailed much more at large in my first geological work, which is known to Mr. Playfair. How then can he suppose *Piedmont* and *Lombardy*, intersected as they are with hills composed of secondary strata, which have participated in the catastrophes common to all, to be covered with the *debris* of the *Alps* carried down by rivers and torrents, when they cut out the vallies in an original mass of greater extent and elevation than its remains, viz. our present Alps? The millions of years required for the production of such effects could not form in his mind any objection against that theory; in which ages are reckoned for nothing; but we have seen in these very countries direct proofs that many ages cannot have elapsed since the sea retired from them,

them, after having produced there secondary strata, which had themselves been subjected to great revolutions, previously to its retreat. Instead, therefore, of embracing the whole of M. de Saussure's descriptions, Mr. Playfair has dwelt only upon such of his remarks as he thinks favourable to his own system. No man was more intimately acquainted with the Alps, and even with the Apennines, than M. de Saussure; but his observations had not been carried sufficiently far in other countries, to enable him to correct some peculiar notions which he had formed respecting the effects of the *débacle*, or the necessary *retreat of the sea*, and of the first currents produced by rains. To these agents he ascribed the origin of the gravel and blocks that cover the low grounds between those hills, through which run the *Po*, the *Tanaro*, the *Sesia*, and the *Tesin*, and on which he had observed the quantity of the loose soil to be greater in proportion to the distance from the Alps. Against my opinion on this subject he had made an objection, a part of which only has been considered by Mr. Playfair. "I do not think" (he had said) "with Mr. De Luc, that the little depth of the "vegetable mould can afford any proof of the small "antiquity of our globe." (This objection is grounded on a mistake, which I shall presently point out; but Mr. Playfair has taken no notice of what immediately follows:) "Not that I differ from him in "the main upon this great question; I have fre-"quently expressed myself to that effect; but I con-"ceive that it must be decided by other argu-"ments." It is with this view that he adverts, as

I have

I have done with respect to other shells, to the oyster found in such a high state of preservation in the mountain of Supergue, which Mr. Playfair was disposed to take for one of those masses of *pudding-stone*, formed on the shores of the sea antecedently to the birth of our continents, at the same time that he ascribes to the latter an antiquity without any assignable limit.

277. With respect to M. de Saussure's remark on the product of vegetation, it is evident that he confounded the *vegetable earth* of which I spoke, with what I had called the *terreau*, or that part of the loose soil to which cultivation is extended. In the plains of Lombardy, as well as in other lands, this loose soil has little depth above the masses of stone which had constituted their surface when they were first abandoned by the sea. In these plains the surface was levelled by the rivers, which, before they were embanked, and while their waters were wandering over the whole of the low ground, deposited on it, as they retired into their bed, the small gravel and powdered materials which they had brought down with them, and of which the quantity continued to increase to the end of their course, where they discharged themselves into the Adriatic. And so far were the plains of Lombardy from having been excavated by running waters, as, for instance, by the Po, that the sediments of this river have, in some places, where it has been embanked, raised its bed above the level of the adjacent lands. Of the soil thus formed in these plains, the surface, which

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is cultivated, is what I have called the *Terreau*; but an original loose soil is also found on the hills, of which sometimes it constitutes nearly the whole mass. I was not therefore speaking of the *terreau*, in the case considered by M. de Saussure, as I had fully explained, but of the *vegetable earth*, that black mould produced by the decomposition of vegetables, on soils that have remained uncultivated ever since the birth of our continents, a stratum always very distinct from the basis on which it has been formed; and its accumulation may therefore truly serve as a chronometer.

278. I have dwelt at great length upon this phenomenon of *blocks* and *gravel* found disseminated over the surface of our continents; but, if I had omitted these details, no just idea could have been formed of its importance in the *history of the Earth*. In order to remove long established prejudices, we must collect, and exactly delineate, the features of the objects distorted through their medium. The first facts which I have adduced, viz. the accumulations of fragments of every kind, that are dispersed in many parts of the earth, where no trace is found by which we can be directed towards any eminence, whence they may have proceeded, prove the absolute impossibility of their having travelled in any manner over the surface, whatever time be allowed for such a migration. The last facts have served to establish the following important point:— that those parts of the primitive strata, which, during the great catastrophes, had sunk the lowest, and on which

which the greatest number of secondary strata of different kinds were accumulated, were also the parts where the catastrophes were the most prolonged; and that the perfect preservation of the sea shells in the latest of these strata, which mostly consist of sand, is among the number of facts of various kinds, which have convinced M. de Saussure, as well as myself, that the termination of these catastrophes by the birth of our continents took place at a period of time not extremely remote. This circumstance, therefore, directly deprives the advocates for the migration of stones over the surface of the land of the resource of *time* for that operation, and necessarily leads us to conclude that they proceeded from the *interior* parts, during the course of those catastrophes, of which the preceding details have ascertained the characters.

279. It thus appears, conformably to what I stated when entering upon this subject, that the phenomenon of the *stony fragments* scattered over our soils constitutes one of the principal geological monuments; and is indeed so forcibly characteristic of the causes which formerly acted upon our globe, that no theory, which does not, by direct inference from some of its fundamental propositions, explain this phenomenon, can be entitled to any confidence. It will not then excite surprise, that these scattered fragments of the strata have formed a chief object of inquiry with me in all my travels: and may I not hope that the facts, which it has cost me so much time and labour to collect in various countries, will

will be read, if not with the design of verifying them on their respective spots, at least with that attention which the importance of the subject so justly claims? Indeed, from my earliest observation of this phenomenon, I remained convinced of its connexion with all those, which ascertain both the state of our continents at their birth, and the causes of that state.

279. *a.* The first and most considerable trait of the original state of our continents, a trait now determined with great precision, is that their whole mass consists only of the ruins of strata, which, from evident characters, must have been formed in a horizontal and continuous position at the bottom of the sea. This acknowledged fact having effectually excluded all those theories in which the birth of our continents had been ascribed to the operation of *slow* causes of different kinds, has led to inquiries respecting the true cause to which these ruins of *marine strata* owe their present elevation above the level of the sea. Now, on this point, as it has been shown, two theories only are admissible; for either the bed of the sea has been raised throughout the whole extent occupied by our continents, or other continents have subsided so low, that the sea has overflowed them, abandoning its ancient bed.

280. And thus we have finally arrived at the most important question in geology, which we now possess all necessary means of deciding. Lazzaro Moro, indeed, together with a few other naturalists, had,

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some time ago, adopted the hypothesis of *elevation*; but geological knowledge had not then been sufficiently advanced to bring the question to an ultimate decision. Some motive, not openly avowed, which has induced many to reject the theory of the change of the bed of the sea, as produced by the *subsidence* of other continents, has led to the revival of that of the elevation of our present continents. It has been revived in Germany, as I have had occasion to mention, by Dr. Schmieder; but although this geologist is acquainted with some particulars of the state of our mineral strata, he is far from having such a knowledge of the subject as was possessed by Dr. Hutton, whose theory, therefore, I have thought it necessary here to examine, having already published an answer to Dr. Schmieder in Germany, where his work appeared.

281. Among other important facts acknowledged by Dr. Hutton and Mr. Playfair, by which they have been led to greater circumspection in the formation of their theory, is the following: that the catastrophes which occasioned the present well known state of our mineral strata were frequently repeated, and at such intervals of time, that new strata were formed on the ruins of the preceding, and themselves afterwards suffered many catastrophes, wherein the strata which they had covered participated. And although Dr. Hutton, not having extended his observations to a sufficient number of countries, had but an imperfect knowledge of this last circumstance; though he was unacquainted with the

the variety of the phenomena which prove these catastrophes to have been continued to a period of time not very distant from our own, yet anterior to the birth of our continents; he was still obliged, in order to account for some phenomena of that kind, which he had observed in Great Britain, to have recourse to the hypothesis of alternate *subsidences* during the intervals of more considerable *elevations*. Now this shows with increasing evidence the impossibility that a vaulted roof, such as our continents must in this case form, should not, at the period of each of the fractures so frequently repeated, have opened a passage to the expansible fluids which had raised it, and thus have fallen down again under the waters of the sea. These obvious consequences were, without doubt, perceived by Dr. Hutton, though he passed them over in silence; and by them he was probably induced to cement with melted granite the inner part of this continental roof. But, besides that *granite* is evidently disposed in *strata* like other mineral substances, and that these strata have undergone the same catastrophes which have been suffered by the rest, this hypothesis precludes, as we have seen, the ascent of *lavas* in the interior parts of the continents; an ascent which is, however, an undoubted fact.

282. Lastly, the phenomenon of the *stones* scattered over the surface of our land furnishes us with a new criterion, by which the two rival theories may be tried. In that of *elevation*, it could not be supposed that these fragments had been thrown up from within

within by the *explosions* of *expansible fluids;* for the liberation of these fluids formed the most direct objection against this theory. After attempting therefore to obviate that objection by the means of melted granite, it was necessary to assume, as had been done in most of the preceding theories, that these *fragments* had been propelled by rivers and torrents along the surface of the already existing continents. To this opinion I had in particular opposed the vallies and chains of mountains, intervening between many spots on which these *blocks* are found, and every eminence of their particular class. This has been one of the reasons for supposing the chains of mountains, and the vallies separating them, to have been so formed by the running of water since the birth of our continents; an hypothesis, however, as contrary to the theory of elevation, as to that of subsidence; since in both, the vallies are to be considered as resulting from the *angular movements* of the very masses which constitute our present chains of mountains. But when Dr. Hutton and Mr. Playfair had recourse to their hypothesis on the action of running water, they thought only of the fragments of *granite* mentioned by M. de Saussure as lying scattered over *Jura* and the plains of *Lombardy*: now, from the details into which I have entered respecting the grand phenomenon of scattered *blocks*, I have shown, that, admitting that hypothesis to account for the particular instances in view, which, as has been seen, is far from being the case, it is still by no means applicable to the multitude of blocks of a great variety of stones, found dispersed

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over so many hills and plains at a considerabls distance from every mountain, without any trace of the track which they might have pursued.

283. This phenomenon, therefore, forms, as I have said, a new criterion of the two theories which alone remain for the explanation of the origin of our continents; at the same time that the decision between these theories must determine what judgment we should form as to the nature of the operations which took place on the globe antecedently to that period. In the theory of *elevation*, according to which the *strata* must have acquired their present ruinous state during the very operation which produced the continents, (a circumstance alone sufficient to overturn the system,) the dispersion of the blocks cannot be accounted for: this, I think, has been abundantly made to appear. In the theory of *subsidence*, on the contrary, according to which the disordered state of the mineral strata was produced at the bottom of the sea, previously to its retreat, this event having been occasioned by the subsidence of other continents, the great phenomenon here in view is easily explained in all its circumstances; as I shall now proceed to show.

284. The caverns, from the formation of which have arisen all the catastrophes suffered by our mineral strata, must necessarily have been filled with *expansible fluids*, were it only with common atmospheric air. Let us consider the enormous pressure exercised upon these fluids by the subsidence of

masses

masses of such magnitude, that those remaining at the highest level have produced mountains by their angular movements, down to their lowest strata. Now as it was only through the intervals of these disunited masses that the compressed fluids could escape, they must have rushed out with an unparalleled impetuosity. Here then is a "*force*," of which we may form a sufficient idea without comparing it "*with that which projected the planets in their or-*" *bits*." It is the same in its nature, though far superior in degree, with the force which raises lavas to the summits of the volcanos of the *Andes*, and by which Dr. Hutton himself remarks that "rocks of "an enormous size are projected.....some miles into "the air;" an effect which was observed during the formation of *Monte Nuovo* near *Naples*. Nor is heat requisite for the production of this effect; for with respect to these phenomena, its agency is confined to the production of the expansible fluids, which become *dense*, only in proportion to the resistance of the columns of melted matters, which they force up, and through which they sometimes find their way. Heat, therefore, must be a very inconsiderable cause of *density* in the *fluids*, and of consequent violence in their escape, in comparison with the pressure of those enormous masses, which could not reach the bottom of the caverns, without driving out these fluids through the intervals which separated them; and the increased resistance, which the fluids encountered from the sides of such masses as afforded the narrowest spaces for their escape, contributed to the *angular movements* of those mas-

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ses, during the subsidence of others that were intermediate, to a greater depth. By this *force*, therefore, which nothing that was detached and loose could possibly resist, the fragments, broken off by the collision of the lower parts of the strata, were thrown up to the surface, in the manner of bombs, notwithstanding the resistance of the water, under which these explosions took place. I have mentioned the collision of the masses as chiefly produced in the lowest strata: and this will lead to an important remark.

285. It is long since mineralogists, observing the relation which *granite* bears to other *strata* in their respective associations, have judged that it must have originally extended under all those strata, together with the mineral substances considered as contemporary with it, viz. *porphyry, pure quartz*, and *gneiss*, which, when associated with schistus, are found beneath it; and hence, undoubtedly, was Dr. Hutton induced to employ *granite* in a melted state, as a cement for the vaulted roof of his continents. This circumstance will explain to us why most of the blocks dispersed over the surface, are of *granite* and other substances of the same class; for, as I have remarked, the collision of the masses during their angular movements must principally have affected the inferior strata. Now here we meet with one of those unexpected verifications, which characterise a true theory. The heaps of blocks found in the same places are composed of very different kinds of *granite*, because similar differences

occur

occur with regard to the strata of granite which lie incumbent on each other. This we may observe in granitic mountains, either in vertical sections, where the strata have nearly preserved their horizontal position; or in a horizontal succession, where they are become vertical or highly inclined. M. de Saussure, when developing his great proof of the *stratification of granite*, has remarked this circumstance in the *Aiguille du Midi*, the vertical strata of which exhibit granite, granitelle, veined granite, and *other stones of the same class;* an expression which implies genera, comprehending different species. The collection of facts of this kind will increase, in proportion to the attention which shall be given to mountains. Thus, M. Pfaff, Professor of Philosophy at *Kiel*, who, being a native of *Wirtemberg*, is thoroughly acquainted with the mountains of the *Black Forest*, the ruinous state of which I have myself observed, informed me, some time ago, that, in passing over an horizontal tract of that country, a league or two in extent, where the strata were nearly vertical, he had observed as great a variety in the species of *granite* as occurs among the *blocks* disseminated over the hills of *Holstein*, and the neighbouring borders of the *Baltic;* countries, however, which are situated at a considerable distance from every granitic mountain, and of which I shall give a description in my Travels.

286. Such then being the nature of *granite*, which, in many places, is found associated with *porphyry*, *gneiss*, and pure *quartz*, or alternating with them

through

through mountainous tracts of great extent, we can no longer be surprised at the variety of the genera and species of the fragments, which, during the subsidence of the large masses of strata, were thrown up to the surface by the expansible fluids, in spots where no strata similar to them are observed, because those from which they were detached are covered by others to a considerable depth. Now I have already remarked that this phenomenon is not confined to granite and porphyry, but that *blocks* are met with of a great variety of other strata, and even of *grit* and *sandstone;* and when naturalists, relinquishing the inadmissible idea of a transportation of materials by running waters, shall form local lithologies, as M. de Saussure has done in many places, the surface of the soil will become much more interesting; since it will serve, in a great measure, to disclose to us the internal parts of our continents. Thus is opened a new field of observation, of which I have been able to trace the outline only; but enough has been said to show that a considerable knowledge of the mineral strata may be obtained, by ascertaining, from external characters, what lies at a great depth below the surface.

287. Lastly, we are to consider the effects produced by water itself in the course of the revolutions which took place at the bottom of the sea. At the time that the expansible fluids were driven outwards with violence, the water was impelled by its own weight to pass inwards through the same openings. We may therefore readily conceive the tremendous conflict

conflict which ensued between these two forces, and how the fragments of rock, which they both encountered in their passage, must, by their opposite action, have been fractured and ground against each other, during that violent agitation. Meanwhile, during the slow descent of the great masses, a large quantity of water made its way through their intervals; but, when they reached the bottom of the caverns, they, of necessity, drove out thence a considerable part of that water, which must have burst forth in torrents at the bottom of the sea. If then we represent to ourselves the combined effects of these successive absorptions and disgorgements of the liquid, and of the trituration of the fragments, first during that conflict, and then in consequence of the excessive agitation of the water which resulted from it at the bottom of the sea, we shall not be at any loss for a cause adequate to the dispersion of gravel or rounded stones over the surface of our present land; a cause of which neither the operations of running waters upon our continents, nor the action of waves upon their shores, can have ever suggested the least idea to those who have observed this phenomenon with due attention.

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288. I have thus taken a general survey of the most important geological monuments; and it is evident that, unless we view them in all their connexions, nothing but vain conjectures can be formed respecting the history of the earth. I hope that I have

have made them intelligible, whether considered in themselves, or in reference to that history, even to those who have not as yet applied themselves to the study of geology. When, however, a general idea of the subject shall have been formed from a first perusal of this Treatise, it will be necessary frequently to return to so spacious a field of investigation, and attentively to consider every part. But, above all, those who devote themselves to the pursuit of the physical sciences must learn to seek after and abide by *facts* alone, rejecting that imperfect and superficial knowledge, which gives rise to endless hypotheses, and which has been productive of so much error on a subject equally important to all men.

289. I could not have determined these phenomena with the same evidence, nor so clearly have pointed out their connexion with the history of our globe, had I not given an exposition of the several opinions entertained by geologists concerning them. Instead of dwelling on systems formerly celebrated, but now nearly abandoned in consequence of the errors acknowledged to be contained in them, I have fixed on that theory, in which a greater number of ancient errors have been avoided than in any other, which contains important truths, and has been formed and illustrated by two men of genius and much information, with considerable detail, and a great display of methodical inquiry. Having thus set the example of sincerely courting discussion upon essential points, I entertain a hope that those, who may

may think my opinions erroneous, and particularly Mr. Playfair, will no longer avoid such discussion; since it is only by fully entering upon it, that the road which leads to real science can be shortened.

290. If the course of the preceding investigations has been attentively followed, it will have been found that they contain nothing foreign from the subjects announced in the title of this Dissertation; namely, *the state of our continents at their birth, and the time which has elapsed since that period.* To these two points must every geological monument be referred; for it is only a knowledge of the original state of our continents, together with the study of general physical causes, that can afford the means of ascertaining the cause to which they owed their birth; and an acquaintance with this state, and with the causes, which are now operating, and must have operated, on our continents, since they have *begun to exist*, leads to the determination of their *age*; while all these collectively open the only road to the knowledge of the *physical events* which succeeded each other upon the globe before the great epoch whence the present abode of mankind derives its date. Such is the extensive field over which I have passed; and I shall now proceed to collect, under one view, the several points which I have, as I think, established on a permanent foundation; as, notwithstanding the length of time and the attention that I have bestowed upon the subject, I have found nothing, either in nature, or in the works of naturalists,

ists, which does not tend to strengthen and confirm them.

I. Our *continents*, at their birth, that is, when they were abandoned by the sea, consisted of the ruins of those mineral strata, which, as far as we have been able to penetrate under the surface, compose their whole mass.

II. This change of relative level between the *continents* and the *sea*, was not produced by their *elevation*, but by the *subsidence* of pre-existing continents, over which the sea immediately flowed.

III. The ruinous state of the *strata* composing our continents was not, therefore, an effect of the revolution which gave *birth* to the latter, as is supposed in the theory of *elevation;* it resulted from successive catastrophes undergone by the strata during the very period of their formation, by chemical precipitations, at the bottom of the sea; and these catastrophes were occasioned by the *caverns* formed beneath them.

IV. The *cavities* of lakes, the *vallies* among mountains and hills, the *abrupt and shattered faces* which those eminences present to the plains, existed on our continents at the period of their birth; having been immediate effects of those eatastrophes, which had reduced to heaps of ruins the mass of *strata* that afterwards became our *continents.* A succession

sion of such catastrophes was continued, till those continents were abandoned by the sea; and since that epoch, all the operations produced on them by the combined action of atmospherical causes, gravity, and running waters, have tended only to obliterate those original characters, by reducing the abrupt faces into gradual slopes, by softening down the asperities of the hills, by raising, instead of excavating, the beds of vallies, and by filling up the cavities of lakes. The present race of men, however, may still, for the use of future generations, define these original characters, from what remains of them in every kind, and may point out the process by which their gradual obliteration is yet carrying on.

V. The *gravels* and *blocks of stone* found scattered in such large quantities on the surface of our continents, nay, even those observed in the beds of rivers flowing in plains, were not deposited there by the operations of running waters, these blocks and gravels being also among the *original characters* of our continents. The gravel composed of *flints* is derived from the decomposition of great masses of *chalk strata*, at some period considerably subsequent to their formation, yet while they were still covered by the sea; the flints only remaining as we now find them. The gravels formed by the attrition of fragments of the strata, as well as the blocks of the same stones, were projected from the interior of the globe by the *expansible fluids*, during the subsidence of the strata, and disseminated over the bottom of the sea by the agitation of the water.

VI. The

VI. The *naked and precipitous cliffs* which border some of the coasts are not to be ascribed to the action of the sea. No such action could have produced abrupt sections on the coasts, where none had at first existed. They are *original characters* of our continents, resulting from disruptions of the strata, at the period of the vast subsidence which produced the new basin of the sea; the action of which directly tends, on the contrary, to obliterate those characters, by forming strands at the feet of the cliffs; which strands, when sufficiently raised and extended, permit the steep faces to be reduced by degradation into slopes.

VII. Neither the *materials* conveyed by rivers to the sea, nor those detached from the steep coasts by waves, are transported to any distance "*along the* "*shelving bottom*" of the ocean; the waves and the tides throw them back upon the coasts, and even bring up the sand from the bottom, wherever they can reach it; so that they deepen this bottom, and propel both sand and gravel towards the beach, until the latter has acquired that degree of declivity, which enables the waters, in retreating, to carry back with them the new materials which they have brought up in rolling toward the shore. In this manner is formed one of those kinds of low lands which are found along the coasts.

VIII. The *sediments of rivers*, both at their mouths and along their course, were at first considerable in their quantity, and were mixed with large fragments;

fragments; as well on account of the ruinous state of the surface, which placed an abundance of materials in the way of the running waters, as in consequence of the operations carried on by their streams in excavating and straitening their channels in loose soils. But these transportations of large materials were gradually diminished, proportionally to the progress of vegetation on the surface when its asperities were softened, and to the restriction of rivers within fixed beds; so that what their waters now deposite, either along their course, or at their mouths, amounts to little else than mire. Our alluvial lands are formed by these sediments of rivers along their course; those which reach the sea contributing to the formation of the new lands, by which our continents are extended.

IX. Since the birth of our continents, the *level of the sea has not changed*, either absolutely, or in relation to the land; for all new lands, which everywhere may be easily distinguished from the original coast, are perfectly horizontal, excepting where they have suffered partial depressions behind dikes; and their level has been determined by the largest waves at the highest tides. This is an evident proof that the birth of our continents was the effect of a single revolution, the continuance of which was so short, that it left no traces of gradual progress, at those points where the new land comes into contact with the original soil.

X. Lastly,

X. Lastly, all the operations, which have had a tendency to alter the original form of our continents, must have *commenced at the epoch of their birth.* The several causes of these operations are known to us, for we still behold them at work; and by studying them with sufficient care we may easily ascertain all that has been effected by them in every spot. In many places we may even trace what each cause has operated within certain known periods of time; a circumstance which supplies us with *chronometers* of different kinds, all independent of one another; and all these chronometers agree in attesting that the age of our continents cannot exceed 4000 years. This fact at once overturns all the fabulous chronologies of several of the pagan nations; and thus are also subverted all the geological systems by which attempts have been made to support these fables. "It "has been deemed an act of courage, and a laudable "proof of freedom from *prejudice*," observed M. de Dolomieu, who was himself in some degree under the influence of prejudice, "to go beyond others in mul-"tiplying the number of the ages which have elapsed "since our continents have been yielded to the in-"dustry of man. Fearless of ridicule, and not dread-"ing the kind of disgrace incurred by those who do "not subscribe to such fanciful and exaggerated "opinions, I may hereafter publish a work, in which "I shall combine *historical monuments* with *geologi-"cal observations*, for the purpose of proving, that, "if we allow 10,000 years to have passed away since "the time when our continents became or were again "rendered habitable, we may even then perhaps go "beyond

"beyond the truth." (Journ. de Phys. Janvier, 1792.) It was to the same purpose that M. de Dolomieu expressed himself likewise in his *Mémoire sur les pierres composées et sur les roches,* already quoted on this subject, § 90. But he was himself seduced by that very opinion, which, as he said, had led so many naturalists to vie with each other "*in multiplying the number of the ages which have elapsed*" since the globe has attained its present state: this appeared both when he allowed 10,000 years for that period, though conscious that such an assumption might exceed the truth; and when, instead of comparing with the theory of subsidence the disorder which he observed to prevail throughout the mineral strata, he had recourse to some *external shock* to account for their fracture and dislocation, and to some *planetary influence* for the purpose of raising tides to the height of 800 toises. (Journal de Phys. Nov. 1791.) At the time when I first became acquainted with him at Paris, in 1782, I had not arrived at that degree of information which I have since acquired; but if circumstances had again brought us together, as we mutually wished, after we had respectively added to the stock of our observations and experiments, of various kinds, I entertain little doubt that he would have acquiesced in all the conclusions of which I have here given a summary.

291. These conclusions, deduced from the objects which have been treated of in this Dissertation, contain also, as I remarked when stating its general subject, the only true data respecting the events

which

which succeeded each other on our globe, *previously* to the *birth of our continents*. I call them *data*, inasmuch as they result from the direct consequences of what has been proved respecting the preceding points; and I shall here subjoin them.

I. All our mineral strata were successively produced by *chemical precipitations* from a *primordial liquid*.

II. From the first precipitations, which constitute the most ancient monument of physical operations, and beyond which our observations do not carry us, proceeded the *strata of granite* and other kindred substances.

III. During the long continued precipitations of different genera and species, the *strata* thus produced underwent a series of *catastrophes*, originating in the successive formation of *cavities* beneath their mass, in consequence of the infiltration of the *liquid* into the interior parts of the globe; hence also resulted a cause of successive changes in the nature of the *precipitations*.

IV. While many of these operations were going on at the bottom of the sea, and subsequently to the formation of *granite* and other *primordiary strata*, there existed other *continents* stocked with *animals and vegetables*. For, although our continents, through their whole extent, must evidently have been occupied by the *sea* until the epoch of its retreat,

we find, both in their inland parts, and on their borders, the remains of terrestrial animals and vegetables, in strata posterior to the primordial.

V. Those vegetables and animals, of which we find the remains imbedded in these marine strata, had therefore originally existed in islands surrounded by the sea, and formed by the disruption of long peninsulas, the result of prior revolutions; during which, a portion of the liquid being absorbed into the interior parts of the globe, had left uncovered those eminences which had been produced at its bottom. Several of these eminences, already stocked with plants and animals, but separated from each other by preceding catastrophes, underwent new catastrophes by simple subsidence, which reduced them below the level of the sea; where the *debris* of the organised bodies remaining on them were covered by marine strata, some of which were consolidated, and others remained soft. Lastly, in consequence of fresh convulsions, which still continued to affect the whole mass from its base, these new strata suffered depressions, fractures, and angular movements, like all the former; a circumstance which characterises the theatre of these scenes.

VI. The retreat of the sea from these parts of the globe was produced by the *subsidence* of those continents whence the vegetables and animals of the islands had proceeded; and some of the islands, which still existed in different parts of the ancient sea, having become the summits of the mountains of

of the new continents, were the principal source of their vegetables and animals.

292. In treating the several objects of this Dissertation, I have not enlarged on these last points, as I have had no view but to show in what manner they are deducible from those which I have fully established; neither have I considered the physical causes connected with them, otherwise than in a general light; because the whole of my geological system is contained in the work which has been already mentioned, viz. my *Lettres sur l'Histoire Physique de la Terre*, addressed to Professor Blumenbach, Paris edit. 1798. This is a system of which the object is so comprehensive, and the parts, though reciprocally connected, require such a diversity of proofs, that, if I had entered into particulars respecting all the proofs of every proposition, the reader might easily have lost sight of those points which had been successively established, as well as of the mutual relations subsisting between them.

293. I have therefore preferred stating the whole system at present in the manner which I have here adopted; a plan which I had been already obliged to follow in my Letters to Professor Blumenbach; because the proofs of the several propositions are of different natures. With respect to the operations which took place on the globe, previously to the birth of our continents, necessarily connected with the *monuments* remaining of the *causes* which acted during that period, the inquiry depended in great part on

on experimental philosophy, and especially on chemistry. But certain fundamental and necessary points in these sciences, furnished by the advances made in some of their principal branches, could not be treated with advantage but by entering into numerous details. I therefore introduced them only under the form of propositions, the proofs of which I engaged to give in a future work; and this engagement I afterwards fulfilled in two works published at Paris; one in 1803, under the title of *Introduction à la Physique terrestre par les Fluides expansibles*, the other in 1804, under that of *Traité élementaire sur le fluide Electro-galvanique.*

294. Another part of the subject consisted in ascertaining what those *monuments* were, the *causes* of which were to be assigned; and this depended on an exact determination of two connected points; first, the *present state* of our continents, and next, the real actions of *physical causes* on them; in order to fix with certainty what was the state of these continents at their *birth;* which state must necessarily have resulted from the action of antecedent causes.

295. These are the objects which I have at present resumed; but, on account of their extent, I have been obliged to divide this subject also into two parts; the first, which has been treated in this work, has consisted in bringing forward all the great geological questions in a form that submits them to the decision of determined facts, here pointed out in general terms: and if these are true generalizations

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of the phenomena of nature, they must be decisive of the above questions. The proofs, therefore, of these generalizations are all that is remaining for me to produce; and they will be found in the account of my Travels, which will thus complete my plan.

296. In order to establish this general theory, it was sufficient to refer to such general phenomena of our continents, as clearly manifest the effects of past causes; and the highest mountains, particularly the *Alps*, have chiefly supplied me with those phenomena, where they have more simple characters, and are less remote from their origin. There we first observe the *schisti*, as having succeeded to the *granite* and other contemporary strata, but evidently after catastrophes, which, in various places, have separated those two classes, by the interposition of *breccias* containing fragments of the earliest class. To the schisti succeed the most ancient of the *calcareous strata*, inclosing the remains of marine animals; but these also were produced after catastrophes, in consequence of which the *primordial strata* having their surface strewed with their fragments, *breccias* were again formed of them, by which, in several places, those strata are separated from the first of the calcareous kinds. To these last succeed other calcareous strata of different species, followed by those of *sandstone;* still however with intermediate *breccias,* each involving fragments of the anterior strata, and indicating catastrophes previous to every change in the *precipitations.* All these alternations are observable in the Alps, and in the adjacent tracts,

tracts, and have been described with great accuracy by M. de Saussure: they likewise occur, with some differences, in other chains of mountains. These phenomena were sufficient to lead to a general theory respecting the events of this long period, as well as to point out the successive causes of these events; and this was the principal object which I had in view.

297. But when we pursue our observations on extensive tracts of country, and on different continents, great varieties occur in the *successions* of the strata. This we might naturally expect to be the case in those wide tracts of hills and plains, where the catastrophes continued during a longer period of time, and the primordial strata are known to exist, only from the blocks and gravels of their several species which were thrown outward during the catastrophes, and disseminated over the surface by the action of the sea; and where consequently, among the great variety of strata produced by the changes in the precipitations after each catastrophe, although in various countries certain successions of the strata are found to be repeated, yet, in general, great differences must necessarily occur. But indeed considerable differences of this kind existed from the beginning of those operations. M. de Saussure, for instance, has shown, with much detail, that, in setting out from the central chain of the Alps towards the opposite sides of those mountains, the strata are found not to correspond; and that, on the side towards *Italy*, they descend much more abruptly, than on that towards *Switzerland* and *Savoy*. There

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is likewise much difference in the successions of the strata in various high chains of mountains, and in the lowest. In many places we meet with *gneiss*, *porphyry*, or the *syenite* of Werner, instead of *granite;* this last, in other spots, appears without the *schisti;* these again are found unaccompanied by the more ancient *calcareous strata* observed in the Alps; and the more recent calcareous strata are often without a covering of *sandstone.* These circumstances indicate that, from the commencement of the operations which we are enabled to trace back by means of geological phenomena, the *chemical precipitations* from the *liquid* originally encompassing the earth varied in different places, on account either of local differences in its composition, before it was agitated by the tides, or of a difference in those parts of the globe itself over which it spread, after the elements which produced it had been penetrated and liquefied by *fire.*

298. On this subject, the progress of observation will undoubtedly throw considerable light. For this purpose it will be necessary to ascertain, in several countries, the succession, or the different successions, of strata, of various genera and species, which are externally discovered, their respective disruptions and inclinations, and the different fragments of stones, whether in gravel or in blocks, which are found disseminated over the surface, and clearly indicate inferior strata. In a word, observations and descriptions, as exact as those which M. de Saussure has given us with respect to the chain of the Alps,

must

must be extended to other portions of the globe; and a similar study must be made of the less elevated mountains, of plains and hills, of the beds of rivers, of lakes, and of the sea shores, under all the different points of view which will be presented in my Travels. Many interesting circumstances respecting these various objects of investigation are already collected in several valuable works; as, for instance, in the descriptions of the northern part of *Asia* by Messrs. PALLAS and PATRIN; and we may likewise expect some very important information, in regard to *South America*, from so zealous and enlightened a naturalist as M. HUMBOLDT.

299. I have no doubt, that, in proportion as these observations shall be extended, it will be the more clearly understood, that our knowledge of the mineral strata is derived entirely from the catastrophes which I have described; as by them have been brought out on the surface, in so many parts of our continents, immense masses of strata, of different genera and species, which, without those revolutions, would for ever have remained concealed from us; being covered by other strata, which we should have had no motive of sufficient interest to dig through, to any considerable depth. To those catastrophes, therefore, we are indebted for geology; the study of their nature and effects has afforded us the first rudiments of this science; by them we have been enabled to ascertain the *original state* of the *continents* now inhabited by man, together with the *time* which has elapsed since they first came into existence;

existence; by them we perceive that the whole observable mass of these continents must have been produced under the waters of the sea, and that, during a particular period of its production, *other continents* existed, which have since disappeared from the surface of the globe; lastly, by them and by observation of the monuments of past effects, which they have thus brought into our view, we have been stimulated to deep researches in terrestrial physics, and more especially in mineralogical chemistry, whence the general causes of these effects have been disclosed.

300. Thus a grand and diversified field of observation and reflection is opened to the activity of the human mind. And I trust that the remarks, to which I have been led in the course of this discussion, may have made it appear that this subject is not to be treated with levity, being one which is now become of such deep importance to all mankind. I have shown this great truth more explicitly in my *Letters to Professor Blumenbach*, than in the present work, particularly dedicated to the *Science of Geology*, considered in itself.

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301. Before I close this volume, there are two objects which claim my attention, as connected with its general subject; these are some experiments made by Sir JAMES HALL, considered both by himself and by other philosophers as confirming Dr. Hutton's hypothesis

hypothesis respecting the consolidation of the mineral strata by the *heat* of *fusion*; and Mr. KIRWAN's work, entitled *Geological Essays*.

Remarks on some Experiments of Sir JAMES HALL.

302. It is well known that the most direct argument against that part of Dr. Hutton's theory, which supposes an excessive *heat* in the interior of the globe, is derived from the *calcareous strata*, which, if they had been subjected to a degree of heat capable of *calcining* them, would have lost their *fixed air*, which, however, they retain. This objection Dr. Hutton attempted to remove, by means of another hypothesis; namely, that the pressure of the superincumbent ocean had prevented the fixed air from escaping. Sir James Hall has thought to strengthen and confirm this hypothesis by a series of experiments, which are now sufficiently known to allow me to confine myself to a short account of the experiments themselves, and of their general results.

303. The chief of these experiments were made in gun barrels, in the breech of which was introduced a small china tube, half filled with calcareous powder, well rammed in; the other half of the tube was filled with pounded silex, in order to prevent the calcareous substance from coming into contact with that, by which the barrel was afterwards to be hermetically closed. This last substance consisted of a mixture

mixture of lead, tin, and bismuth, which melted at a heat little above that of boiling water. When the barrel had been welded at the muzzle, the breech was introduced into a muffle, in order to make it undergo a considerable heat. The part of the compound metal which was thus exposed to melt, by being nearest to the heated part, was kept separate from the china tube by confining within the barrel a small quantity of air, assisted by the vapour of a very small quantity of water; but the rest of the metal continued in a solid state. After the calcareous matter had endured that degree of heat, and the barrel had been allowed to cool, the fusible metal was entirely removed from it by the application of a moderate heat; and the china tube was then taken out, for the purpose of examining the calcareous substance. Sir James afterwards changed the form of his experiments, using only china tubes, and employing other means for closing the part of these tubes which was not exposed to a great heat.

304. In the course of the years 1801, 1802, and and 1803, Sir James Hall made a number of experiments, by the various methods which he more fully describes, amounting to 156; and the following are the general results, as stated by himself. "In an "operation so new.....constant success could not be "expected; and in fact many experiments failed, "wholly or partially. The results, however, upon "the whole, were satisfactory, since they seemed "to establish some of the essential points in this "inquiry. These experiments prove, that, by me-"chanical

"chanical constraint, the carbonate of lime can be
"made to undergo strong heat, without calcination,
"and to retain almost the whole of its carbonic
"acid, which, in an open fire, at the same tem-
"perature, would have been entirely driven off......
"By this joint action of heat and pressure, the
"carbonate of lime, which had been introduced
"in the state of the finest powder, is agglutinated
"into a firm mass, possessing a degree of hardness,
"compactness, and specific gravity, nearly ap-
"proaching to these qualities in a sound limestone;
"and some of the results, by their saline fracture,
"by their semi-transparency, and their suscepti-
"bility of polish, deserve the name of marble."

305. In order to determine the degree of resistance, which, in the course of these experiments, the fixed air and the aqueous vapour underwent at different degrees of heat, and their effects upon the calcareous matter, Sir James Hall produced the resistance, by means of moveable weights, in the manner of the valves applied to the boiler of the steam engine; and the following are the principal results: That under a pressure equal to 52 atmospheres, or 1700 feet of sea-water, *heat* is capable of changing a calcareous powder into a common limestone: That under that of 86 atmospheres, answering nearly to 3000 feet, or about half a mile, a complete marble may be formed: and lastly, That with a pressure equal to 173 atmospheres, or 5700 feet, that is, little more than one mile's depth of sea,

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the carbonate of lime is made to undergo complete fusion, and to act powerfully on other earths.

306. Such is the substance of Sir James Hall's experiments; I entertain no doubt respecting their accuracy; and, in common with all experimental philosophers, I consider them not only as manifesting much genius and persevering industry, but as opening a new road in the important investigation of the various effects of fire, under different circumstances, on the substances of our globe. But are these experiments applicable to Dr. Hutton's hypothesis? This is a very different queſtion, which I ſhall now proceed to examine.

307. The present case is entirely analogous to that of which I had treated in my Work, entitled *Idées sur la Météorologie*, § 553, & seq. published in London, in 1787. The case to which I allude related to the theory of *evaporation*, which I had stated in former works, and which was as follows: " that its *product*, from the lowest temperature to " the higheſt, is always of the same nature; viz. an " *expansible fluid* composed of *fire* and *water*, having " a *specific gravity* less than that of the *air*, and " consequently, as soon as it is formed, mixing with " the *air*, and rising in the atmosphere." In opposition to this theory were adduced experiments made upon *aqueous vapour* under the *pressure* of the atmosphere, in enclosed spaces, where it could overcome this pressure only by the temperature of *boiling water*. Messrs. LAVOISIER & DE LA PLACE, in particular, urged some experiments which they had

had made on evaporation at the upper part of a barometer. In these experiments, the *aqueous vapour* produced by a drop of water, above the column of quickfilver, at the temperature + 10 of my scale, (about 54° of Fahr.) depressed that column only half an inch; which indicated, as the maximum in that temperature, an *expansive force* equal only to the pressure of a column of quicksilver of half an inch. When the pressure was increased by causing the column of quicksilver to rise in the tube, as soon as it arrived within half an inch of the top, the *vapour* began to yield; and when the column had reached the top, was again reduced to a drop of water.

308. Of this kind were the experiments which were adduced against my system; it being from them inferred, that, since the *vapour* produced by a temperature lower than that of *boiling water* was entirely decomposed by the *pressure* of the *atmosphere*, common *evaporation* could not be of the same nature; —but must be, as maintained by M. Le Roy, (whose opinion had been adopted by the greater number of philosophers,) a *dissolution* of *water* by *air*.

309. I replied, in the abovementioned work, that these experiments had no connexion with the case of *evaporation* in *air*; as in them the whole *pressure* of the atmosphere was made to bear on the *vapour* alone, by the interposition of a substance *impervious* to it: That, in this case, the *vapour* could be formed only when the degree of resistance which it under-

went

ment was not superior to that of its own *expansive force,* always correspondent to the density which can be produced by the actual *temperature:* That the laws of this correspondence of its *densities* with the *temperatures* were the same which determined the point of *ebullition*, and continued to prevail in the different *degrees of heat* of *boiling water* under different *pressures;* because, the *vapour* being then to be produced in the *water* itself, it could not be formed, unless it could also acquire such *density* as to enable it to overcome *alone* the *pressure* actually exerted upon the *water;* which necessarily required a certain *degree of heat:* but that in *open air*, as soon as the *particles* of *vapour* were formed, they penetrated it, and were mixed with it, and thus supported no greater *pressure* than they were capable of resisting; the rest of the *pressure* being sustained by the *air* itself.

810. Such was my general reply to this objection; but, having resumed the whole subject of *Evaporation* in a Paper presented to the Royal Society, and inserted in the Philosophical Transactions for 1792, I there fully stated the *laws* of *aqueous vapour*, beginning with the lowest temperatures, and proceeding to the highest degrees of heat of boiling water; and I proved, by direct experiments, that these *laws* were the same both in *air* and *in vacuo.* This theory has hitherto remained unanswered; and thus the notion of the *dissolution* of *water* by *air*, a notion which had introduced great error into *meteorology*, and through the latter into general *chemistry*,

has

has been rejected by every attentive natural philosopher.

311. This case, as I have already said, is perfectly analogous to that of the application of Sir James Hall's experiments to Dr. Hutton's hypothesis. When *calcareous substances* are *calcined* in open air, the *fixed air*, which is produced, immediately escapes, and it continues to form, until the substance is deprived of its ingredients; but when a *solid body* prevents its escape, the particles first formed acquire a degree of condensation proportional to the resistance which they experience; and they oppose in their turns the same resistance to the formation of other particles. Under such circumstances, therefore, if the *heat* be increased, it produces other combinations of the *fixed air* with the *calcareous earth*, as in the experiments of Sir James Hall. But under *water*, which the particles of that *gas* can easily penetrate, in which, collecting in bubbles, they rise rapidly, on account of their inferior specific gravity, there can be no impediment to their formation; any more than to that of the *aqueous vapour* in water, under the pressure of the atmosphere, when the heat is sufficiently intense. And indeed at sea, while calm, whatever be the depth, *bubbles* are very commonly seen ascending in the water; and that sometimes in great abundance, owing probably to the putrefaction of animal substances at the bottom. This, it is true, is referable only to the immediate pressure of the *sea*; and it might be thought that the *strata* themselves had exerted an immediate pressure upon each other.

But

But I answer, on the support of facts, that, in numerous places on our continents, strata of *calcareous stone*, or of *chalk*, constitute the surface; and that these consequently, while they were forming in the sea, could not have been subjected to any other *pressure* than that of *water*.

312. Dr. Hutton's hypothesis therefore receives no support from the experiments of Sir James Hall; and I shall now proceed to show that they would serve, on the contrary, to overthrow it, could it maintain itself upon any other ground. What these experiments directly prove, is, as might have been expected, that *expansible fluids* cannot be formed when the substances which should produce them, are under the immediate *pressure* of a *solid body*. Now how could such fluids have been formed beneath the mass of *strata* which they were to elevate, to say nothing of the pressure of the ocean? Perhaps, however, it may still be objected that such *fluids* are, notwithstanding, actually formed *under the strata*, since it is by them that *lavas* are thrown up to the surface. But in that case, these fluids are enabled to form themselves, because the matter of the lavas, being soft, yields to them, successively retreating and escaping through some opening; a circumstance perfectly unconnected with the notion that *expansible fluids* can be formed beneath the whole extent of *strata*, which rest upon the very substances whence those *fluids* ought to be produced. Here then the effect is checked at its commencement; but should it be supposed to proceed to its completion,

viz.

viz. the *raising* of our *continents*, that operation alone would suffice to overturn the whole theory, as I have already shown. For, supposing that, contrary to the above fact, the *fluids* could really have formed and forced up the mass of our strata, together with the ocean, the moment that the crust should have been broken, the fluid would have escaped, and the dislocated mass must have fallen down again into its former place.

REMARKS ON MR. KIRWAN'S GEOLOGICAL ESSAYS.

313. The second subject to which I proposed to advert was the geological system of Mr. Kirwan; on which, however, I need not long dwell, because the points connected with it have already been discussed in this volume. I entertain a great personal respect for Mr. Kirwan, with whom I had the satisfaction of being acquainted during his residence in London. Besides, our object in geology is the same—that of pointing out the errors of the systems which are in opposition to the Book of *Genesis*, the only authentic source of the knowledge possessed by men respecting their own origin, and that of the universe. This he has done with great success, in regard to such of those systems as he has examined. But he has himself formed a theory, respecting which we differ on many points, and that for a general reason, which I shall begin by stating.

314. I acknowledge Mr. Kirwan's great talents in chemistry, although we disagree respecting an essential

sential point of the general theory of the science, viz. the *nature* of *water;* whence has resulted the difference of our mode of considering the first physical operations which took place upon our globe. I am not less aware of his great knowledge of the details of mineralogy, and of his extensive acquaintance with the works of all the best writers on that subject. But, although *chemistry* and *mineralogy* form essential parts of *geology*, they do not constitute its basis. Nothing, indeed, in the fundamental principles of this science, must be in opposition to that which is clearly demonstrated by particular phenomena: but the *history of the earth*, which is the object of *Geology*, is ultimately resolvable into the *history of the mineral strata and of the sea*; and a knowledge of this history cannot be attained without the attentive study, in various countries, of their hills, mountains, vallies and plains, and of considerable extents of different coasts; a study, to which Mr. Kirwan has applied but little. Being thus indebted for the greater part of his information to the various descriptions of others, it has been impossible for him to discern in them, those defects, which would have been apparent to such as had studied nature with their own eyes; he has not, I mean, been able, in consequence of his own observations, to distinguish the descriptions which merit confidence, from such as, by their imperfections, may lead to error. The application of this remark will presently become evident.

315. That

315. The fundamental difference in our opinions, whence all our other disagreements arise, originates in the conclusion drawn by Mr. Kirwan from the Book of *Genesis*, that the *Deluge* took place on the very *continents* which we inhabit; an opinion which has influenced his whole system. I have pursued a different course, as may be seen in all my works. I began by studying geological monuments; from which I clearly saw that our continents had formerly been the *bed of the sea*, and had become *dry land*, in consequence of a *revolution* which had taken place on the globe. Upon referring afterwards to Genesis, I could easily judge that it was this very revolution which had produced the Deluge; the continents previously inhabited being then *destroyed*. I here only point out this essential difference in our mode of conducting our researches, as I have elsewhere dwelt at large upon that subject; and I am the less obliged to do so at present, because Mr. Kirwan, in opposition to a system so extensive as mine, and founded throughout upon observation and experiment, has merely urged a few objections, comprised in four pages, (p. 61—64,) to answer which it would be necessary to repeat, on each head, all that is contained in my works respecting them, to which he has not adverted. It will moreover be seen that such a recapitulation is not essential to establish the truth of the following remarks.

316. Although we differ respecting the nature of the first physical operations which took place upon

 our

our globe, we yet both of us set out from a period when it was encompassed by a *liquid*, in which were held in solution by each other, all the elements of the substances now observable on the earth's surface. But whereas, in my system, the chemical precipitations produced in this liquid, were collected at first in *strata* at the bottom of the sea, and became *mountains* in consequence only of the catastrophes which they underwent, Mr. Kirwan (p. 21) ascribes to these precipitations the immediate formation of some *mountains*, which he calls *primitive*. It will now appear that, from this first difference in the judgment which we have formed in regard to the most general facts, result all the other differences between our respective systems. If Mr. Kirwan however had observed mountains as long, in as great a variety of places, and with as much attention, as I have done, and had afterwards perused M. de Saussure's *Voyages dans les Alpes*, to which he so frequently refers, he could not but have been struck with the strong light thrown by that work upon the nature of those eminences; which were evidently at first laid in *strata* nearly horizontal and continuous, and were converted into mountains, solely by the catastrophes which befel them. To M. de Saussure's remark on this subject we shall more particularly revert; but, for the details, I must refer to the work of that great observer, more especially to Chap. XXI, entitled, *Poudingue de la Valorsine*, and to Chap. XXXIV. entitled, *Le Cramont*, § 916 & seq.

317. Again,

317. Again, Mr. Kirwan's theory, as well as my own, represents the quantity of the *liquid* as having diminished on the exterior of the globe; and this supposes that internal caverns either already existed, or were subsequently formed. Mr. Kirwan has adopted the first of these ideas, which he developes and supports, p. 24 & seq. It is to the operation of preceding volcanoès, that he ascribes the existence of caverns, into which he believes the liquid to have gained admittance, at first rapidly but afterwards more slowly, through the numerous rifts occasioned by the volcanic fires; and thus the height of the *sea* was reduced first to about 8500 or 9000 feet above its present level; at which time it began to be peopled with fish. During this period were formed, according to Mr. Kirwan, the mountains which he calls *secondary*; I have also employed this expression, but I have applied it, not to the *mountains* themselves, but to the *strata* formed subsequently to the production of *organic bodies*. He supposes however that, at that period, almost all the substances capable of being chemically precipitated had been separated from the liquid; and he employs for the formation of the *secondary* masses, such materials only as were furnished by the disintegration of *primitive* mountains in the sea; whereas, according to my theory, the chemical precipitations continued to take place in the sea, until the birth of our continents.

318. Having formed immediately into the mountains, which he calls *primitive*, the substances che-

 mically

mically precipitated, Mr. Kirwan necessarily considered the *detritus*, supposed by him to have proceeded from them, and containing organic bodies, as having been also originally formed into *mountains*. These last, which he terms the *secondary mountains*, he considers as having been formed around the former, and over those parts of them which were already sufficiently destroyed to admit of their being completely covered by the latter; because it is thus that we at present find them. Now he could never have believed these eminences to have been originally formed into mountains, were it not that he had seen but very few of them, and that he had not had the opportunity of observing those among them which are impressed with such characters as clearly denote, that, before they were *mountains*, their masses had been disposed in horizontal and continuous *strata*.

319. And here it will be proper more particularly to speak of the remark of M. de Saussure which I have already mentioned. Had mountainous regions, and especially the *Alps*, been as familiar to Mr. Kirwan as they are to me, that remark could not but have made the same forcible impression upon him, which I had experienced from it. On the subject of *secondary mountains*, I never varied in opinion, because their nature is very intelligible; but, although my acquaintance with the Alps had been earlier than that of M. de Saussure, it is to him that I am indebted for understanding the *structure* of the mountains of this class, which I contented myself with

calling

calling *primordial*, and not *primitive*, at the same time confessing that to me their nature was involved in mystery. From my previous knowledge, however, of the spots, I was well prepared to understand the theory, of which I have already given an exposition, but which, in this place, it will be necessary again to state.

320. In the first volume of his Travels, M. de Saussure, speaking of the nearly *vertical* strata which he had observed, and considering them as having been produced in a *solid state*, ventured upon an opinion, in which I could not acquiesce, viz. that they had been formed in that position; and such is the opinion entertained by Mr. Kirwan; as also by M. Werner, to whose theory what I am here going to state will therefore equally apply. The regular strata of *breccia* of the *Valorsine*, which Mr. Kirwan mentions, though without stating the remark subjoined by M. de Saussure to his description of them, were those which so forcibly struck that observer, when he came to consider that strata, which showed by the fragments of other stones contained in them, that they must have been at first in a *soft state*, could not have been formed in a *vertical* situation. Conceiving, therefore, that they had been originally *horizontal*, or nearly so, and yet observing them to be enclosed between other strata lying in the same position as themselves, viz. those of *granite* toward the central chain, and of *schistus* on the opposite side, he thence concluded, that these must all have

been

been produced in the same *horizontal* situation as the strata of breccia. When afterwards, viewing from the summit of the *Cramont* the several chains of those mountains, he saw the *calcareous strata* of the exterior ridges turned up against the *central chain*, and considered that these strata, which are perfectly parallel to each other in their present situation, must nevertheless have been originally in a soft state, since they contain marine bodies, he entirely relinquished his first opinion; and only saw, in that assemblage of mountains, from the exterior ridges to the central chain, strata which had been at first successively formed *upon* each other, though of different classes and species, in a *horizontal* and continuous situation; and which had afterwards been converted into chains of mountains, in consequence of some revolution, during which they had been *fractured, dislocated,* and reduced to their actual position. Thus it is that he has diffused upon geology a light, which can never be obscured; because whoever shall at any time attentively study the nature of mountains, as I have done since I have had that light to guide me, in a variety of countries, will everywhere find the same phenomena, which, through the eyes, irresistibly speak to the understanding. Of this Mr. Kirwan has not been aware, because he has not sufficiently observed with his own eyes those parts of our continents which consist of mountainous tracts; and it will be seen that the least defensible part of his theory is to be ascribed entirely to the want of that necessary information.

321. Mr. Kirwan again believes with me, that, during the formation of the first of the mountains which he calls *secondary*, (those containing *marine exuviæ*,) the *liquid*, continuing to pass into the interior parts of the globe, diminished considerably on the surface; he supposes, p. 44, that this retreat continued until a few centuries before the *deluge*. And here also we agree on two points: that our continents were once the *bed of the sea*, and that the waters of the sea produced the *deluge*; but after this our opinions do not any farther coincide, and for the following reasons.

322. If Mr. Kirwan, in studying the history of our mineral strata, and of the sea, had really consulted the objects themselves; if, in regard to the former, he had been thus led to conclude, in common with the most accurate observers, that they had been originally formed in a horizontal and continuous situation, and that they now constitute *mountains*, in consequence only of the catastrophes which they have undergone, he would not have sought in the *deluge* an agent to which should be ascribed their present *form*, an agent that had no share whatsoever in its production. If again, in regard to the *sea*, he had studied in different places, and on coasts of different kinds, its real, and at the same time its only possible, operations, he could not have attributed to the deluge such effects as the divulsion of vast continents, and the separation of large islands from the main land; and more particularly the scooping out of the immense bed of the *Atlantic*. The

deluge,

deluge, beyond all doubt, was a miraculous event, and in this light I have considered it: but when some cause of that event, as produced by the Divine will, has been pointed out, it must be shown that the effects of that cause are in conformity to the ordinary course of nature, and consistent with what we observe on the earth's surface. These necessary conditions are admitted by Mr. Kirwan, and in his theory he considers them as fulfilled; it is therefore under this point of view that I must proceed to examine it.

323. I shall first state our respective theories, in reference to that great event, beginning with my own; because my exposition of it will leave me nothing farther to add. According to the first part of my system, which is purely geological, those portions of the bottom of the ancient sea, which became our continents, had, before they were abandoned by that sea, been brought, by the natural operations which I have detailed, to the state in which we now observe them; and since that event they have undergone no change, but such as is effected by the causes at present acting on the earth's surface. By studying the respective actions of these causes, which are of several classes, in order to discover what they have produced, we may, with certainty, determine the *state* of our continents at their *birth*. All this, I observe, is wholly independent on any reference to the book of *Genesis;* and it is the same with respect to that part of my theory, which concerns the manner in which our continents became *dry land;* geological

logical facts clearly proving that this event took place in consequence of a *revolution* upon the globe, during which, the *sea*, changing its bed, overflowed the *continents* which then sunk. There needs, therefore, no commentary, to show the conformity between *Geology* and the history of the *Deluge*, a catastrophe, which God himself, when he "saw "that the wickedness of man was great in the earth," had announced to Noah in the following terms: "Behold, I will destroy them *with the earth*;" which *destruction*, as we learn from the description of the event, was occasioned by the *sea*, in Scripture called the *deep*. *Geology*, however, extends still farther its agreement with *Genesis*; it ascertains with precision, as I have shown in this work, the *state* of our continents at their *birth*, and the *causes* which from that time have acted upon them; whence two other essential points are established; one, that the *level of the sea* has not changed since that period; the other, that the *antiquity* of our continents is not greater than the *Mosaic chronology* from the Deluge ascribes to them. Such is a summary statement of my system, with which the *Deluge* is indeed connected, but not as a proof; the system being otherwise supported by a series of *facts*, embracing the whole physical history of the earth, from the period at which it was covered by the primordial liquid, to our own times.

324. Very different is Mr. Kirwan's system, both in its origin and in its progress. Having erroneously collected from some expressions in the book of Genesis,

nesis, of which he had not sufficiently studied the connexions, that the Deluge, though occasioned by the *sea*, had taken place on the continents still inhabited by men, he experienced the same insurmountable difficulty in reconciling his explanation with geological phenomena, which had been felt by those who had preceded him in the same attempt, and whose systems, easily refuted, had caused the anthority of Genesis itself to be questioned: since it had been imagined that to overthrow those systems was to subvert the authenticity of the sacred volume. Of this danger Mr. Kirwan was not aware; because, not being sufficiently acquainted with the phenomena exhibited on the earth's surface, he had overlooked those descriptions of them which were the most accurate, and dwelt on such only as seemed to denote violent attacks of the sea upon the land, and in particular upon the mountains; and in order to account for that supposed effect, he imagined that the part of the *liquid*, which had penetrated into the interior of the globe, had issued from thence in a miraculous manner, and produced the deluge. This is stated by Mr. Kirwan in p. 67 and seq. He supposes that these waters proceeded from the great southern ocean, below the equator, and thence rushed on the northern hemisphere, covering thus once more the *mountains* which had been originally formed in the bosom of the sea; and that afterwards this superfluous quantity of *liquid* returned with equal impetuosity to the south, and again passed into the interior of the globe. I make no objection to such motions of the water; they constitute the *miraculous* effect

supposed

supposed by Mr. Kirwan; but then it is requisite that he should satisfactorily account for the geological monuments connected with this part of the subject. I shall therefore proceed to take a short survey of those which he considers as explained by the foregoing cause, and to show that his opinion entirely proceeds from the confidence which he has placed in vague and imperfect descriptions.

325. The first phenomenon which struck Mr. Kirwan was the ruinous aspect of numerous hills and mountains; an aspect vaguely considered by some geologists, as denoting catastrophes produced by a violent attack of the sea. But if he had himself examined many of those hills and mountains, or had placed his reliance on accurate observers, he would have recognized the unequivocal proofs which I have enumerated in this work, not only that catastrophes of a very different kind had there taken place, but that they had occurred in succession, at periods greatly antecedent to the birth of our continents, and considerably distant from each other.—The second phenomenon which engaged his attention was that, among the extraneous fossils found in our regions, were the remains of animals, both marine and terrestrial, species analogous to which exist at present in the southern hemisphere only; and he adopted the opinion of M. Pallas in supposing this effect to have been produced by a sudden motion of the sea from south to north, correspondent with his own idea of the Deluge. I shall speak first of the *marine* bodies: these are the remains, not only of animals now belonging

longing to no seas but the southern, but, in a still greater quantity, of such as do not at present live in any part of the ocean. Besides, these remarkable fossils of the marine kind are contained almost exclusively in *stony strata*, which, in various countries, constitute great mountains, such as the whole chain of *Jura*, and the external chains of the *Alps;* and it is by the fractures, partial subsidences, and angular movements, which those strata have undergone, that they have been converted into *mountains*. Now these are not effects which can be ascribed to the Deluge.—With regard to the quadrupeds, at present inhabitants of only the southern regions, the bodies of which are notwithstanding found buried in our climates, and are supposed, both by Mr. Kirwan and M. Pallas, to have been transported by a sudden motion of the sea from south to north, I confess that I could never understand how M. Pallas, who, from his own observations, knew *Asia* to be intersected to a great extent, by chains of hills and mountains, could have conceived the bodies of rhinoceroses and elephants, which are so much heavier than water, to have floated over those chains, instead of being detained in the first vallies. But let us suppose them to have reached the northern climates, together with the waters rendered turbid by the loose soil which they must have washed from the surface of the land: what would have been the result? Nothing but large accumulations of mud, with those bodies dispersedly intermixed. Now I have seen bones of elephants, taken from several spots which I have myself observed: the soil was composed of

regular

regular strata of different species, lying one over another; and thus plainly denoting, as well as some of which I am going more particularly to speak, that the sea was *calm* during their formation.

396. One of the phenomena upon which Mr. Kirwan lays the greatest stress in his theory, had been already many times adduced as a proof of the Deluge, but, as long as it was thought to prove *directly* that event, it had given occasion to unanswerable objections; I mean the vast accumulations of *shells* found in loose and superficial soils. Mr. Kirwan considers these shells as a proof of that *return* of the *sea* upon our land, to which he ascribes the Deluge. These scattered marine bodies forming, however, in themselves a very important phenomenon, my brother and I had directed our attention to it, from the time when we first began to devote ourselves to geological pursuits: we made a large collection of the shells contained in loose soils; not merely with the view of arranging them in our cabinet, but for the purpose of examining the spots where they had been deposited, and all the circumstances attending them. The results of our observations are, for the most part, detailed in my first geological work, where Mr. Kirwan may see them; I shall therefore here confine myself to a general outline. These shells are found in regular strata of marle, argil, and different kinds of sand. In some places, those strata continue the same to the greatest depth we can reach; in others, their nature changes as we penetrate the soil; and then it most

commonly

commonly happens that, after having met with a certain *family* of *shells* in a particular class of strata, we find different families in those either above or below; in the same manner as we observe the families to change in the *stony strata*, where they form whole mountains. Lastly, the loose strata, which are always parallel to each other, are in various places fractured and highly inclined, as well as the stony strata which they cover. Such were the observations, which, at an early period, convinced my brother and myself,—first, that, at the same time that these shells directly prove our continents to have been formerly covered by the sea, it is no less evident that they could not have been deposited by the Deluge; for, in that case, the utmost confusion would prevail throughout all the loose substances on the surface of our soils;—secondly, that these regularly disposed strata were produced from the last precipitations which took place in the sea, when marine animals existed at its bottom;—and thirdly, that the sea retired from our continents, by changing its bed.

327. I shall not dwell upon some strange effects ascribed by Mr. Kirwan to the supposed return of the sea upon our land; such as producing the angular form of the continents towards the south, excavating the immense basin of the *Atlantic*, and separating *Great Britain* from the continent of *Europe*, and *Ireland* from Great Britain. Mr. Kirwan could have been induced to suppose such operations of a liquid, by his persuasion only that the continents which

which we inhabit, must necessarily be the same with those which belonged to the antediluvians, and by his preconceived notion of the original form of these continents. For nothing can lead us either *a priori*, or from the general phenomena of the earth, to determine what were the original *outlines* either of the first or of the present continents; though the form of their *surface* clearly manifests the causes by which it was produced: and to attribute their actual outlines to a sudden irruption of the sea, and to its residence upon the land during the short period of the Deluge, implies great inattention to the facts on which the question ought to rest, namely, the real effects of sudden percussion, and of the residence, however prolonged, of a liquid mass upon solid substances, such as are the *stony strata* which compose the greater part of our continents.

328. Neither shall I dwell on the phenomenon of the organised *terrestrial* bodies, *vegetable* as well as *animal*, which are found among fossils; for this subject, when considered in all its branches, is connected with so many others, that it could not be conveniently introduced in this place. Mr. Kirwan says only a few words, p. 64, on the very extensive part of my theory concerning this subject, developed in my preceding works; and he does not even consider the phenomenon in its geological point of view, but contents himself with opposing to me the authority of Genesis, which book he does not however seem to have studied with sufficient attention. I have not therefore to defend my system against any precise

cise objections; and with regard to his own theory on the same object, to have demonstrated by all the other phenomena, even those on which he particularly grounds himself, that the sea has never overflowed our continents since the period at which it first abandoned them, is surely sufficient to show the necessity of seeking some other cause, to which those terrestrial vegetables and animals found among our fossils may be ascribed.

329. This is all that I consider as requisite to be said here of Mr. Kirwan's geological opinions, which, from the beginning, I have treated as very distinct from his chemical and mineralogical knowledge, wherein he is very eminent. I do not, however, here take leave of the subject itself. My present undertaking, in its whole extent, is intended to embrace geology, considered in a general sense, with all the principal questions which have arisen in that science. This first work I have more particularly devoted to the objects connected with those two great and fundamental points in the history of the earth, the *state of our continents at their birth*, and the *causes of that state*. What I have advanced on these points, having been the immediate results of the *general phenomena* which I have adduced, would have sufficed for the establishment of fixed and permanent bases in geology, if conviction could be commanded by facts presented under such a form. But I showed from the beginning, that this effect could not be expected, on account of the very different systems respecting the same objects, which their authors

authors equally ground on what they pretend to be general phenomena.

330. This circumstance forms the greatest obstacle to the advancement of sciences which, from their objects, can be founded only upon facts, although such sciences might seem to be the least exposed to that cause of retardation; and as it is by the various modes of representing the same facts that the retardation is produced, there can be no other means of overcoming this obstacle, than by a sufficient number of well authenticated details. When we think that we have established a system upon real facts, which however, to facilitate the discussion, we have been obliged to present under general forms, whatever apprehension we may entertain of not being read by those who content themselves with taking a superficial view of science, we must not hesitate to bring forward a great number of well described instances in each of the various classes of those facts, in order that the generalizations may immediately arise from themselves. When natural philosophers shall follow this regular mode of developing their discoveries, (nor will they neglect it, if they find it to be expected from them,) many chimerical systems will vanish even from their own eyes.

331. The fundamental facts in Geology consist of every thing that natural history has already collected, or can still collect, by attentive observation, respecting the *actual state* of our globe; and an accurate

generalization of each of the several classes of these phenomena can be formed only by means of the multitude of *particular phenomena* belonging to it, collected in many different spots, with all their various combinations. This, as I have announced, is the method which I shall pursue in furnishing the proof of the general facts brought forward in this volume, for the purposes, both of establishing my own theory, and of supporting my arguments against those of others: and the performance of this engagement will be found in the account of my *Travels*, which will soon follow the publication of the present work.

APPENDIX.

SIXTH GEOLOGICAL LETTER

TO

PROFESSOR BLUMENBACH,

BY

M. DE LUC,

Being a Physical Commentary on the

ELEVEN FIRST CHAPTERS OF GENESIS.

SIR,

1. IN my last letter I collected together the leading phenomena of a numerous class, tending to ascertain beyond a doubt, the following points. First, that *the birth of our continents* must have been preceded by a *sudden* revolution, during which the *ancient* CONTINENTS *sinking down, formed a new bed for the sea*; and, secondly, that the epoch of this revolution is not more remote than that of the *deluge*, according to the *Mosaic computation*. What then becomes of that immense *antiquity*, to which certain Asiatic nations lay claim, and of which some Geologists have availed themselves, to form systems as groundless as these *Chronologies*?

2. While *Geology* has made advances in unravelling the history of our *Globe*, it has been strongly supported by the enquiries of learned men into those *mythological fables*, which covered with the veil of fancy the true history of *mankind*. In the year 1776, MR. BRYANT published an eminent work (*The Analysis of Ancient Mythology*) in which, tracing by a most laborious and learned analysis, the *Mythologies* of *Greece* and *Rome*, up to their *Egyptian* and *Asiatic* sources, he proves that they all allude to the history of the *Deluge*, as related by MOSES; referring in common to that *event*, defined by the same characteristic circumstances,

 the

the *Epoch* of a *Renovation of the human race,* by a *personage* conspicuously described, who was *miraculously* preserved *with his family,* in a *vessel.* These results, as far as they relate to the nations of *Asia,* have been since confirmed in the three volumes hitherto published of the *Asiatic Researches,* the fruits of the learned enquiries of a most important literary society established at *Calcutta,* under the presidency and direction of the late Sir WILLIAM JONES. Lastly, in MR. MAURICE's publication on *the history of Hindostan,* we have a recapitulation of all that has, through a length of time, been discovered on this important subject, connected with the modern discoveries, and accompanied with many very interesting remarks.

3. Hitherto, however, it might appear, that we have ascertained with more precision only, a resemblance, which unbelievers had already noticed between the Pagan *Mythologies* and the book of GENESIS; whence they had concluded, and still continue to conclude, that the latter was also a *Mythology* compiled by the *Hebrews* from notions which they had adopted during their captivity in Egypt, and from the opinions of the several nations by which they were surrounded; but the *history of the earth,* necessarrily connected with that of *mankind,* will decide that dilemma. The cosmogony of GENESIS, at the same time that it contains, in common with the cosmogonies of these nations, the great event of the *renovation of the human race* after a *deluge,* instead of carrying back the era of this event to an immense distance, assigns to it only a very small number of ages. Here then is an indisputable *criterion:* we cannot refuse to acknowledge, that, of these compared cosmogonies, that of which the chronology is ascertained, must be the *original.* Now I have shown in my former letter, that the phenomena of the earth confirm, beyond all doubt, the *chronology* of the Book of GENESIS. If then to this first criterion of *truth,* is added this striking circumstance, that though composed at a time when *Polytheism* prevailed among every other nation, GENESIS represents the *Israelites* as professing the most pure *Theism,* such solid foundations will, I hope, induce every lover of truth and of mankind, to examine attentively the whole of what I shall further state, relating to the characters of this great Book.

4. The

4. The Deluge is described by MOSES under circumstances so precise, that if they are true, they must be impressed on the whole of our globe as forcibly as its *chronology:* and now, in proving that they are so, I shall not confine the character of MOSES to that of a *faithful historian*, but shall make it manifest, that he must necessarily have *been directed by God himself.*

5. No account of events of such considerable magnitude, could be more simple than this of MOSES: the history of *mankind* is the main series of events which he is to impress on the memory of the people entrusted to his direction; marking those in which the intervention of GOD had been manifested to their forefathers. He first, therefore, expounds to them, in a succinct manner, the successive operations by which, at the word of GOD, the *earth* was prepared for the reception of *man*, but with such leading circumstances, that we have found them clearly impressed on our globe: then, proceeding in his main purpose, the *history of man*, and arriving at the era when *the human race was renewed after a deluge*, during which GOD granted his protection to NOAH and his family, he relates this event: after which he confines himself to the history of that *family*, with whom the repopulation of the globe commenced. In this narration, MOSES does not stop to explain or prove the events he treats of, but simply relates them; the *Israelites* knew, from *tradition*, the truth of a great number of the circumstances related; and they admitted without inquisitive curiosity, those they could not know, because MOSES exercised among them a *supernatural power*, which proved him to be the minister of GOD. We have not now the benefit of such direct proofs, but the phenomena of our *globe*, which have been the immediate effects of what he declared to the Israelites, are for us as decisive proofs as what they witnessed.

6. I shall begin with the ALMIGHTY's *Revelation* of himself to NOAH previous to the *Deluge* (GENESIS, *chap. vi. v.* 13.) " And GOD said unto NOAH, the end of all flesh " is come before me; for the earth is filled with violence " through them: and behold, I will destroy them *with the* " *earth.*" The more literal translation of the latter part of the verse is, " I will *destroy them*, and the *earth with them.*" We see that the term *earth* does not here signify the *terres-*

trial

trial globe, but the *land* inhabited by man; conformably to this we read in chap. i. v. 10; "and GOD called the dry land "*earth*." It was therefore the destruction of those *continental* parts which was foretold to NOAH. Now *Geology*, as I have explained in my former letter, proves, that at a period corresponding with that assigned to the *Deluge*, ancient *continents* sunk; and as the *sea* rushed over them to occupy their place, all the *organised beings* necessarily perished. Thus Geology, a science but very lately advanced so far as to explain to us the real history of the globe, comes in, as an evidence, that, at this very period, the *human race* that inhabited the *earth*, or the former *dry lands*, was *destroyed with it*.

7. Let us, however, suppose for a moment, that the history of the Deluge, thus shown to be real, is only the record of a *tradition*, true as to the events, but into which has been inserted, by fraud or superstition, a pretended *revelation* from GOD to the family saved. Then we must necessarily admit, that the Deluge surprised this family as it did the rest of mankind; that by some fortunate accident they found themselves shut up in a *vessel* with abundance of provisions; and that this vessel, instead of being swallowed up in the chasms which were opened in the *old continents*, (though it would certainly have happened in the natural course of things) floated in such an extraordinary manner as to be at length stopped against one of the *islands* of the former bed of the sea, before it had entirely abandoned it: in such a case, what could have been the facts observed by this family? That after an astonishing rain, during forty days and forty nights, their bark had been set adrift; that in this state it had been violently driven about on a body of water terribly agitated; that during a certain period they saw nothing but water under the horizon; that in process of time, a raven and a dove gave them notice of their approach to *land*; that at length their vessel rested on a *mountain*, where they disembarked; from which moment the waters gradually retired from the land on which they found themselves.

8. This, I say, must have been all that any spectators, accidentally preserved in such a catastrophe, could have observed or described; it never could have entered into their thoughts, that this dreadful catastrophe had been caused by the sinking of an immense extent of *land*, whence a *new land*

was

was formed from what before was the *bed* of the sea: it is *Geology* that unfolds to us that great revolution, written on our globe, in indelible characters; and it thus necessarily recals to our mind, that important circumstance which we had by our supposition set aside for a moment, namely, the *prediction* of the Deluge to NOAH, purporting, that all the *inhabited parts of the globe should be destroyed.* Here then is a revelation confirmed by indisputable facts; and this first demonstration might serve to overthrow all the arguments of unbelievers against any divine revelation; but we shall see that the whole Book of GENESIS bears the same character.

9. Not only the family of NOAH was struck with this event in the manner in which Geology teaches they must have been; but they knew, and transmitted it to their posterity, that GOD had interposed on this occasion, and that it was by his power they had been preserved: we know this, from the ancient MYTHOLOGIES, the first foundations of which necessarily refer to traditions of NOAH's family; for the chronology of this family being confirmed by the phenomena of the earth itself, we cannot doubt that all the traditions must have proceeded from them. Now the nations of the east have applied the whole strength of their imaginations to describe a terrible agitation of *the Sea* during a *Deluge:* or rather, it is from the greatness of the ideas preserved among them, on which they exerted all the power of their fancy when they were left to themselves, that proceeds the strong character observed in the oriental images: and they had not lost sight of the circumstance of a *superior power* presiding in this catastrophe; for they particularly attribute to such a Being, the preservation of a *bark* (notwithstanding the violent agitation of the ocean) containing some *holy person,* with his family, consisting of *seven people:* we may see this particularly explained in MR. MAURICE's *History of Hindostan, p.* 351, &c.

10. MOSES was not indebted to these sources for his information; else he could not have avoided using their imagery: neither did he write from a knowledge acquired in *Geology* or *Physics*; for these sciences had as yet no existence; the observations on nature had till then been chiefly directed to the common wants of human life; and even the origin of what little science they possessed was as much

wrapped up in the veil of mythology, as that of the new race of man. The narration of MOSES was extremely simple, and all it contains is at this day confirmed by *Nature*: I proceed to make this evident with regard to the first circumstance in this relation, namely the *prediction of the destruction of the ancient continents*; and in this discussion I shall point out the geological facts recorded by MOSES, after I have reminded you of some phenomena, from which we have received our first instructions in Geology.

11. I have explained in the course of my former letters, the causes and the effects of certain great changes which took place in our *atmosphere* during the formation of our globe, such as we see it at present. The causes, which are connected with the production of our *mineral strata*, are the succession of *expansible fluids* that issued from the caverns within the globe, at each revolution that happened at the bottom of the *liquid*, the residue of which is our *sea*; the effects, as far as regards the *atmosphere*, are to be traced in the successive changes which befel the race of *marine* animals, and the several tribes of *vegetables*. The last revolution of our globe, that which produced the Deluge, was of the fame nature with thofe, and of considerable extent: thus the atmosphere must have, at that time, undergone a great change; and I have shown a direct proof of it, in the remarkable circumstance of divers species of terrestrial and marine animals, ceasing to exist without the tropics, which existed there previously; and even in the extinction of some species of the latter.

12. I must still recall to your mind a very important fact, now uncontrovertible in meteorology, and of which I have treated in many of my works, namely, that *rain* is not, (as it was commonly thought) merely the condensation (owing to cold) of the water raised into the atmosphere by evaporation. By a process connected with the *rays of the sun*, the vapours ascending in the atmosphere, are gradually converted into *air*; for they disappear to the test of the hygrometer, which, however, as long as they do not change their nature, indicates not only their presence, but their quantity. Rain then is produced, by the *decomposition* of a certain quantity of *atmospherical air*, operated by some *fluid*, which probably proceeds from the surface of the earth: that portion of decomposed

composed *air* suddenly returns to the state of *vapour*, which being too dense for the parts to remain connected, is decomposed itself, first into *clouds*, then into *rain*. This is a material point in the natural philosophy of the earth, of which I have treated at length in my works on this subject.

13. I now return to the Deluge. The *rain* of *forty days*, and *forty nights*, described by MOSES, as an extraordinary phenomenon, was one of the effects of the change which was operated in the atmosphere by the expansible fluids, that escaped from the internal caverns at the beginning of the catastrophe; and this was the prelude to those meteorological operations which brought the atmosphere into its present state. But from this extraordinary fall of water resulted only a first inundation of the *habitable parts of the globe*, and MOSES does not confine himself to this cause only; for he brings into action the *fountains of the abyss*, which, according to the language of scripture, means the sea. These are the causes which MOSES indicates only in a few words; we shall however trace their characteristic effects, in his so remarkably simple narrative of this great event.

14. After these words, [GENESIS, *chap. vii. v.* 17.] "And the *Flood* was *forty days* upon the *earth*; and the "waters increased, and bare up the *ark*, and it was lift up "above the earth;" (which part relates only to the continuance of the *rain*;—and here would have terminated the inundation, had it been produced by the rain only;) it is said [vv. 18, 19,] "And the *waters prevailed*, and were in-"creased greatly upon the earth. And the ark went upon "the face of the waters. And the *waters prevailed exceed-"ingly* upon the earth; and *all the high hills*, that were "under the whole heaven, were *covered*." This expression, "*under the whole heaven*," signifies only *the whole horizon of the inhabited lands*; for the *sphericity* of the earth was then, and for a long time afterwards, unknown to its inhabitants.

15. It was then by the extraordinary *rain* which succeeded the disruption of the caverns under the lands that were on the point of being destroyed, that the ark was set afloat: after which these lands sunk by degrees, and the *sea* flowed in from all parts; by which cause, though the *rain* had ceased, "the

" the waters *prevailed* and were *increased* greatly upon the " earth." It was thus that all the mountains of those parts of the earth were submerged, and even overthrown into the caverns; and the ark would have been carried along by some of the currents and ſwallowed up in thoſe chaſms, had it not been for the *divine interposition,* which is the principal point with MOSES, in his narration, and the emblems of which are found in all the monuments of ancient Mythology. The ark then floated *miraculously* against the currents of the sea, and being borne up towards the place which the waters were abandoning, rested upon an island, which soon after became one of the *mountains* of the new continents. Thus, with the help of Geology, we already trace in this part of MOSES' concise narrative, that it was the sea which flowed in to cover the ancient lands: and now, by consulting Natural History, we shall find also in the rest of that narrative, that the retreat of water there mentioned, was that of the sea, which abandoned its ancient bed.

16. I need not prove, that *our continents have been the bed of the sea;* there are not two opinions on this subject among naturalists, but we are to examine some of the consequences of their becoming dry lands. When the sea changed its bed in the revolution of the Deluge, all the hollow parts which happened to be in the new lands, remained at first full of its waters: but soon the waters that fell in *rain* were added to these; and in every part where the extent of land from whence these waters descended into hollow places, was very great in comparison with the lowest space, whence it could not naturally come out, the superabundant water flowed over at the lower parts of that bason, after having been mixed with the *sea water* which it contained; so that by degrees the rain water took the place of the salt: and thus it is that in the greater part of our lakes, the water they contain is the same as that of the rivers which flow in. But on these new continents, there were also vast basons, where the rain waters which came into them were not sufficient even to compensate for the evaporation that took place at their surface; by which means the quantity of the original water was diminished, instead of being augmented; and this decrease continued till the extent of the stagnant water became so reduced, that there was an equilibrium between the water produced by the rains, and that which was carried off by evaporation, by which

which cause the stagnant water remained salt. Such is indisputably the origin of our *salt lakes*, such as the *Caspian Sea*: for all the systems that have been imagined to explain their saltness, as well as that of the sea, by supposing a continual lixiviation of the lands, and the calculations made accordingly, by which the antiquity of our continents appeared to be millions of years, have had the same fate as those in which it was attempted to explain their formation by *slow causes*: they have vanished before the direct proofs of the small antiquity of the present state of our globe.

17. Among the circumstances of the Deluge, those which regard NOAH himself, his family, and the ark, are the more important, as from these unbelievers have hitherto raised the most specious objections, in the minds of men as little informed as themselves; but we shall find, on the contrary, from the increase of real knowledge, that these very circumstances are those which most conspicuously demonstrate that this narrative contains nothing but truth.

18. If MOSES (as Deists pretend) had formed only a Mythology, upon the model of those which existed in his time, and from some inconceivable motive, had affected to contradict them with regard to the antiquity of the new race of men, he would not, at the very first outset of his history of that race, have committed so gross a mistake, as to make a dove bring to NOAH an olive leaf from a *mountain*; for the Israelites must have known that this tree is never found on mountains; and by placing the Deluge at so short a time back, he lost the means of concealing his mistake, had it been one, under the veil of time. TOURNEFORT, in the description which he has given of Ararat, as a Botanist, has not failed to mention that no olive trees grow there; and this remark alone has made many unbelievers. Ararat, however, and the other mountains, were at the time of MOSES in the same state as they are now, and TOURNEFORT's remark would not have escaped the Israelites, or MOSES himself, had he been writing a fable, but he spoke of the epoch of the landing of NOAH on this mountain; which, according to Geology, was a time when less than a year had elapsed since it was an *island* in the former sea. I shall remind you of the importance of this distinction, after I have considered some other analogous circumstances in MOSES' narrative.

19. We find, chap. ix. v. 3. of GENESIS, that GOD said to NOAH and his family, after their descent from the ark, "Every moving thing that liveth shall be meat for you, even "as the *green herb* have I given you all things." Does not this last expression represent the family of NOAH as surrounded with *verdure* on ARARAT? If, however, MOSES had been writing a fable, would he have again made the mistake of representing as covered with *verdure*, the summit of a mountain issuing from those waters which had prevailed over the *highest* of them? It required but very little attention to judge, that in such a state of things, the family of NOAH, on their quitting the ark, would have found nothing but *mud* over the whole grounds. But the Israelites, to whom he addressed himself, knew from their own traditions, that these parents of the new race of mankind found both *herbs* and *trees* on Ararat.

20. We further find at v. 20 of the same chapter, "And "NOAH began to be a *husbandman*, and he *planted a vine-"yard*." MOSES, neither in this place, nor in any other part of his narration, aims at giving the history of the Vegetation or Cultivation of these new Lands; and in fact it would have been unnecessary, in addressing himself to the Israelites of his times, since they knew it from their tradition: the *vine* therefore is mentioned in this place, only because of the verse following, where, continuing to speak of NOAH, he adds, "and he drank of the *wine* and was *drunken*:" a circumstance which gave occasion to setting forth the characters of his sons, and had consequently considerable influence on the events that followed with respect to this race of mankind. But we are no less here informed of two important facts; one, that NOAH found the *vine* on the same mountain whence the dove had brought the olive *leaf*, and which is also represented as covered with verdure; the other, that he, immediately after his landing, applied himself to *husbandry*, one of the first acts of which was to transplant the vine into a place probably lower than where he first found it.

21. My attentive reader will already be able to foresee how geology serves to explain these great points in the MOSAIC history; but before we proceed to this commentary, I shall show why, in giving his account of these circumstances as

as connected with the history of Noah and his family, he had no occasion to be more explicit than he has been.

22. We have seen in general, that the ancient mythologies were entirely founded on certain traditions of the deluge; thus we may be able to judge from these, what the Israelites must have known from their own particular traditions, and what therefore it was not necessary that Moses should relate at large. Now, we first find in the *emblems* of Heathen nations, and even among the objects of their *worship*, the *dove* flying towards the *ark* with a branch of *olive*. Moreover the great personage of whom their mythologies make mention as *miraculously* preserved from a *flood*, and as having offered up on a *mountain* the *first sacrifice to the Supreme Being*, (a circumstance mentioned by Moses in his account of Noah) is spoken of in them, under different names, as the *first cultivator of the earth*; he who first *tamed the bull* and submitted it to *the yoke for ploughing*; he who first planted *the vine*; and lastly, the first instructor of his race in *arts*. Here then we have all these circumstances mentioned transitorily by Moses; the *olive branch*, the *vine*, the *renewal of agriculture*, and a *first sacrifice* on a mountain, preserved in the *traditions* of the *Pagans*; and as these traditions were the only source of their knowledge of past events, we shall see that they have further consecrated in their mythologies other circumstances which Moses, keeping close to the necessary objects of his narration, did not happen to mention, as it was not necessary to recall them to the memory of the Israelites, but which we shall find also confirmed by natural history.

23. Moses, in his admirably simple narratives, no where mentions, what geology enables us to comprehend, how greatly the family of Noah, as well as their immediate descendants must have been struck in observing how the high parts of *mountains*, stocked with *animals* as well as with *plants*, were serving to propagate *life* around them on grounds which did not bear any appearance of having ever been dry lands. It must however be from their accounts of this fact, that the Pagans obtained a knowledge of it, and these accounts must have been accompanied with very marvellous circumstances, to exalt the imaginations of their successors (not of the family of Shem) so much as to lead them to the conception and admission of certain beings, who had obtained

tained from the Deity permission to *churn* the waters of the deluge by the whirling of a mountain, till the water of life should be recovered, and come to flow down its sides to reanimate expiring nature. Here doubtless is a gigantic effort of imagination; but we see the foundation of it in geology; there is, however, among those flights, a circumstance that at first appears as extravagant as the former, which I nevertheless consider as having been transmitted to them by their ancestors, as they relate it; and it is such, that by strongly exciting their imagination, it must have had great influence in the production of their mythologies. In describing that violent agitation of the *ocean*, true in itself, though they assign it to a fabulous cause, they mention that there issued forth *clouds of smoke* and *torrents of fire.* Now I have reason to believe, from the monuments of *volcanic eruptions* of which I have treated in my 4th Letter, that of the volcanic hills of our continents, many of those which are not encircled by strata produced in the sea, as well as many of the volcanic islands scattered in several parts of our present sea, and which in many places form Archipelagos, were formed during the catastrophe of the Deluge, and that thus the family of NOAH witnessed those portentous symptoms, and transmitted them to their posterity; though MOSES makes no mention of them, any more than of the violent agitation of the water; because the Israelites knew of these facts from their own traditions, as much as the descendants of HAM and JAPHET.

24. We may now judge of the importance of the works of Mr. BRYANT and of the Asiatic Society; since, by disclosing the real essence of the ancient mythologies, they have served to dissipate the obscurity which, through the Jews' forgetfulness of their earliest traditions, hung over the book of GENESIS, even among themselves; and how much we are indebted to Mr. MAURICE, for having supplied us with a collection of these fundamental and other documents in his History of Hindostan, where we find, particularly at pp. 341—354, all the circumstances I have mentioned, together with the following, which, considering this loss of traditions among the Jews, is of very great importance to us. The same personage who in these mythologies is pointed out to us by so many characteristic traits to be NOAH, is still further commemorated in them, as having brought forth with him from his *bark* a quantity of *seeds* which he had there *preserved* in order

order to renew their races after the Deluge. Now, natural history also supports this tradition; for we see by experience, that the preservation of the plants, most useful in our fields and gardens, depends on *culture;* they gradually perish if left to themselves, and are no longer to be found among the *spontaneous plants*. This tradition then, is a true ray of light, which dispels the difficulties that false interpretations of the book of GENESIS have successively produced; for we not only have here a new information, on the means by which *vegetables* were renewed after the Deluge; but we learn at the same time, that even before that event, *cultivation* was necessary for the preservation of the same plants on the *old continents;* a conclusion that agrees with the sentence pronounced to Adam on his being driven out of Eden, which is a material point in MOSES' narrative, and to which I shall return, under the guidance of other facts.

25. As long as it was supposed, that on quitting the ark, NOAH and his family inhabited the same lands which had existed before the Deluge, all was difficulty: for, without speaking of the Deluge itself, which, under such a mistake became incomprehensible, it was impossible to conceive that the smallest blade of grass could have been preserved, not only under a body of *salt* waters which covered the highest mountains, but on a bottom, which, from the top of the mountains to an unknown depth in the plains, is but one mass of strata in the greatest confusion, full of the remains of terrestrial vegetables, and marine animals. Such a gross contradiction between the supposed sense of GENESIS and the *facts*, was well calculated to produce unbelievers; but it was the error of the interpreters of Scripture, who, after the true traditions had been effaced from the minds of the Jews, substituted their own conjectures for the plain sense of the expressions used in GENESIS. At the very beginning of his description of the Deluge, MOSES relates that Revelation of GOD to NOAH, which announced that the *lands then inhabited should be destroyed*. It was not then upon those *lands* that the ark rested, but on *new continents*. When afterwards MOSES comes to his short account of NOAH and his family quitting the ark on Ararat, and mentions the olive tree, the green herb, and the vine, these circumstances in themselves were no part of his information; they referred to other objects; and he had nothing

thing to fear from the criticism of the Israelites, since these particulars must have been known to them through tradition, as they were to the Pagan nations.

26. Now to what period of time are we to assign these different circumstances common to the account of MOSES and to the ancient mythologies? Must we refer them to an epoch so immensely remote as these mythological systems suppose? I have already answered this generally, by proving the small antiquity of our continents themselves; but I shall now proceed to point out a striking connection between one of the proofs of this great truth, and those very parts of the Mosaical history of the earth, which unbelievers have pretended to be manifestly fabulous.

27. I have proved from geology, that before the Deluge, the *summits* of our present *mountains* were *islands* in the primitive sea; and I at the same time observed, that being then in the lower part of the atmosphere, they enjoyed a temperature fitted for all kinds of vegetation. Now in the revolution which the Deluge produced, the sea, by changing its bed, sunk considerably lower; the atmosphere therefore subsided with it, and the former islands, become now the summits of our mountains, attained to a *cooler* part of the atmosphere. If then NOAH and his family found the *olive tree* and the *vine* on Ararat, as well as other plants which subsist there no longer, it is because they had not had time to suffer from the change of their *situation* with regard to the atmosphere: but as by agriculture these plants (together with those the seeds of which NOAH preserved with him in the ark) came to be propagated in lower grounds, the temperature of which suited them, they gradually decayed in the colder region, and were replaced by a greater multiplication of those plants which were more capable of enduring it. Now it was at the same period, and owing to the same cause, that a phenomenon of another kind commenced, which alone would be an uncontrovertible proof of what I have stated in respect to the vegetables, as it continues to proceed before our own eyes; I mean the growing *accumulations of snow and ice on our higher ranges of mountains.* This phenomenon, I say, leaves us no room to doubt, that the summits of our mountains have changed their situation with regard to the atmosphere; and when I referred to it in my

my preceding letter, I then observed, that by comparing the whole mass of *ice* produced from the birth of our continents to our own times, with the course of its progress in times successively known, it is impossible to assign to them an antiquity more remote than that which would accord with the account of MOSES; in which respect this phenomenon corroborates all the other natural chronometers.

28. Thus, in the resemblances we trace so clearly, between the ancient *mythologies* and the *history* of MOSES, resemblances too marked and too numerous not to show a connection between the sources of these different traditions, *geology* now unveils the truth. It is MOSES, and MOSES only, who has transmitted to posterity the true account of those times. He deduced it from an infallible authority, even that whence *nature herself* proceeds, which now bears evidence to his history; and the resemblances we discover between it and these mythologies, proceeded from the *traditions* of the sons of NOAH, whose authentic recitals of what they had observed during and after the Deluge, roused the imagination of the descendants of Ham and Japhet, when they were separated from the posterity of Shem, and thus deprived of the instructions of MOSES.

29. Thus again, are all those *psychological* fables set aside, in which, by a vain analysis of the human understanding, at a period when it was already *instructed*, some philosophers have pretended to derive from the *powers of reason* alone, all the religious notions scattered among the nations of the world: for we now see, that the *origin of idolatry* is to be referred to the debasement of a pure *theism*, the foundation of which was laid in the immediate instruction from GOD to the first race of men, and repeated in HIS revelations to NOAH. But the *worship* of GOD instituted by NOAH at his descent from the ark, was successively corrupted into the *worship* of NOAH himself, into that of his *sons* and their first descendants, and even into the worship of the mere *emblems* by which the several circumstances of the Deluge were represented; and when once men were left to their own imaginations, with respect to those objects, the true meaning of which they could not understand, without a reference to their real origin then obliterated among them, no bounds

could be put to the excess of deviations in the few, and of credulity in the many: a degradation, of which Mr. BRYANT has traced the progress and the causes, in the propagation of the *Asiatic* and *Egyptian* mythologies among the *Greeks* and *Romans*. Nevertheless, beneath the veil of all these deviations, the primitive notion of a SUPREME BEING, to whom these GODS of their own invention were subordinate prevailed at all times among the Pagans: a strong instance of which, as far as relates to the *Indians*, we have in Mr. MAURICE's History of Hindostan, p. 359. What becomes then of all the speculations on a pretended derivation of *theism* from the *human understanding*, since it is thus ascertained, that a true *theism*, proceeding from *revelation*, did exist among the first progenitors of the present race of mankind?

30. Besides these circumstances so clearly proved, which directly give the sanction of *truth* to the recital of MOSES, there are others which will serve to demonstrate more and more, that the sacred historian, in impressing on the Israelites, as a rule for their conduct, the sublime ideas of the benefits, particular commands, and judgments, derived from GOD, did not dwell upon circumstances which they already knew by tradition from their ancestors. If his history had been a *fable*, MOSES might well have been expected to show as much imagination, as those painters who, from false ideas of the Deluge, have drawn pictures in which men are represented crowding to the tops of eminences, and flying from rock to rock to evade the waters. MOSES then, representing to himself Mount Ararat, as covered with the *dead bodies* of the inhabitants of the country who had sought refuge there, would have set it forth in the colours of poetry, and an elegy would have supplied the expression of distress introduced in the pictorial representations. But no images of *death*, no hint of any *mournful scene*, are found in his description; because he wrote the true history of NOAH and his family, who, landing on an *island* of the ancient sea during its retreat, could not find there any trace of the doom inflicted on their fellow inhabitants of the perishing lands.

31. This, however, under a more general form, was the ground of one of the arguments of unbelievers; who, setting out from the false ideas which prevailed on the nature of the Deluge,

Deluge, objected to the history of MOSES, that, if it had really taken place, we ought to find in our strata, *human reliquiæ*, as well as the remains of *terrestrial animals*; which, however, is not the case. But MOSES says expressly, that the *lands* occupied by mankind were *destroyed;* and *Geology* confirms this fundamental circumstance. Thus, far from this want of *human remains*, on Ararat, in the description of MOSES, and among the organized bodies found in our strata, being an objection to the truth of the sacred history, it is a very remarkable confirmation of it. With respect to the *carcases of terrestrial animals* found in these strata, they were, as it has before been explained, buried there by the waters of the sea, while it still covered our continents, and consequently before the deluge.

32. Of all the mistakes produced by abandoning the *literal sense* of GENESIS with regard to this great event, that which has produced the greatest number of unbelievers, is the extent which has consequently been given to the command from GOD to NOAH, with respect to the preservation of *animals*. If the waters, as was imagined, had in effect covered the highest mountains *all over the globe*, it must necessarily have followed, that every animal now existing, must have proceeded from their respective couples preserved in the ark; and so it has been conceived. I shall not stop to notice the difficulties and improbabilities which arose from such an interpretation, they are well known from the commentaries of unbelievers; but let us pursue the history of MOSES, to see if their arguments against his commentators, prove any thing against himself.

33. The passages which they interpret in this manner, begin at p. 19 of the 6th chapter of GENESIS, where God says to NOAH, " And of *every living thing* of all flesh, two of every sort shalt thou bring into the Ark, *to keep them alive with thee;* they shall be male and female." Here doubtless is a *generality* in the expression, which we also find in the subsequent passages relating to the same subject: but here is one of a different kind expressed in page 21: " And take thou unto thee *of all food that is eaten*, and thou shalt gather it to thee; and it shall be for food for thee and for them." Lastly, let us come to the descent from the Ark. GOD first says to NOAH and his family (Chap. ix. ver. 3) " *Every mov-*

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ing thing that liveth shall be *meat* for you; even as the *green herb* have I given you *all things*." Now whoever would not seek after difficulties where there are none, will merely find in these *general expressions*, compared with each other, a form of speech very common, not only among the *oriental* writers, but in *every language*, when a certain *totality* is to be expressed, which the circumstances serve to explain without ambiguity. NOAH was not mistaken with respect to the orders which he thus received from GOD; they were so expressed as to enable him to comprehend, what *animals* he ought to take into the ark, *to keep alive with him*, and also what *provisions* were necessary for their food during the *deluge*. It is not in the short account of MOSES that we can expect to find these details; it is evident through the whole of his narration, that with respect to such circumstances as were known by *tradition* to the Israelites, he simply alluded to them in few words: and we have here a direct proof of it; for if he had related to the Israelites circumstances of which they had no previous knowledge, he ought at the beginning of the account of the deluge, where he mentions its prediction to NOAH, to have indicated its *duration*; which surely was revealed to him, since it was necessary he should be informed of it, in order to proportion the quantity of provisions to be taken into the ark. We see then, from the silence of MOSES on this important point, that he did not think it necessary to enter into such details with the Israelites, because he had no objections to apprehend from them.

34. Lastly, all doubts on this head vanish when we come to the following passage, containing one of the declarations of the ALMIGHTY to NOAH, after his quitting the ark (Chap. ix. vv. 8, 9, and 10.) "And God spake unto NOAH, and to his sons with him, saying, and I, behold, I establish my covenant with you, and with your seed after you; and with every living creature that is with you, of the fowl, of the cattle, and of every beast of the earth with you; from all *that go out of the ark*, to *every beast of the earth*." Does not this repetition of the words, "with you," joined to the expression of "all that go out of the ark," corresponding with the order given to NOAH, "two of every sort shalt thou bring into the ark, to keep them alive with thee," establish an evident distinction, between the species of *animals* which NOAH had taken into the ark, and which had *come out of it* *with*

with him, and "*all the beasts of the earth?*" Here, *Geology* teaches us, why the Israelites sought no such sense in these expressions, as the commentators did who began their interpretations by losing sight of the nature of the deluge: they knew that after the retreat of the waters of the flood, a number of *animals* descended from the *mountains*, and dispersed themselves over the surrounding countries, in proportion as they found food; as we have it figured in the ancient Mythologies, by the *water of life*, which began to flow from the sides of a *mountain*.

35. Thus, setting aside the animals immediately needful to man, and those which, for particular reasons, NOAH was commanded to take into the ark, (the raven for instance) the new continents were peopled with *animals*, as well as vegetables, from their mountains. It is thus that we find an explanation of the phenomenon of the carcasses, found in our superficial strata in the northern parts of the globe, of animals which now exist only between the *tropics*. At the time of the deluge, these animals existed in our climates; we know this, among other proofs, from the quantities of *elephants teeth* that have been found in certain parts of the North, so well preserved as to have been used for ivory, and by the carcase of a *rhinoceros* found in *Siberia*, which, by the account of M. PALLAS, had still a part of the skin with the hair attached. Such of these animals as, at the birth of our continents, happened to be on the tops of the new mountains, spread themselves, as all the other species of animals did, over the adjoining countries. But the *animals* underwent the same fate as the *plants*; they propagated only in places where the new state of things afforded what suited them; and hence it is that each distinct region is found now to have its peculiar plants and animals; a circumstance of considerable moment in Geology, which I shall explain more fully in some other work, in treating of the *origin* of *organized beings*.

36. Every part of this sublime narration of MOSES, is impressed with characters that mark it as proceeding from the great Author of nature himself. I shall continue to notice the circumstances of that solemn *covenant* which GOD vouchsafed to make with the inhabitants of the new lands, by considering the *sign* annexed to it, (Gen. chap. ix. vv. 12 and 13.) "And God said: this is the *token* of the *covenant* between me and you, and every living creature that is with

you,

you, for perpetual generations; I do set my *bow* in the *cloud*, and it shall be for a *token* of a *covenant* between me and the earth." I shall first show what this object presents to our notice, considered as it stands connected with *natural history* and *Geology*, after which I shall speak of the proofs we have of the reality of this event.

37. I have already had occasion to remark, that *rain* does not proceed from the condensation of the *waters* raised by *evaporation*, but that it is produced by a chemical process, in which a portion of the *atmosphere* itself being decomposed, returns to the state of *aqueous vapour*, which, by its abundance, first forms *clouds*, and at length, the fall of water, which we call *rain*. In the present state of our globe, we observe two very different sorts of *rain*: the one which prevails over a large extent of country, either in a calm, or during regular winds; this *rain* is commonly foretold by the fall of the mercury in the barometer; it is always of some continuance, and is not accompanied with any other particular phenomenon: this I shall call *simple rain*. The other is more local, and the barometer seldom announces it: its effects are sudden, and return by fits; it is always accompanied with *gusts of wind*, which are also local; often it ends only in *showers*, but sometimes it is attended with *hail, thunder, lightning*, and even with hurricanes. I shall call this *tempestuous*. Now it is to this latter kind of *rain* that the *rainbow* (or *Iris*) belongs; for it requires, that, at the same time when the air is clear in the part of the horizon where the sun happens to be, there shall be in the opposite part a *cloud* very low and very thick, and that another *cloud* shall be so situated as to produce *rain* between the thick *cloud*, and the spectator looking that way; circumstances that never happen in the case of *simple rain*, the clouds belonging to which are much elevated, and extend at once over a large tract of country: it then also *rains* or *snows* on the highest mountains; while in the case of *tempestuous rains*, we need not go very high, to have a tempest below us: and when the former kind of *rain* comes to cease, the clouds everywhere break and disperse at once without partial showers.

38. Thus, in order to decide certainly the question, whether the *rainbow* was known to the *Antediluvians*, it would be sufficient to know whether at that time there were any *tempestuous*

tempestuous rains: but all we know in this respect, is, that there is no mention of *hail* or *thunder* in all that MOSES has said of the times anterior to the deluge. This does not amount to a proof, but it authorizes an inquiry from *physical* and *geological* principles, in order to judge whether there be a possibility that the *rainbow* had not appeared before this revolution on our globe.

39. The *simple* and the *tempestuous rains,* unquestionably proceed from one common cause: that is, the decomposition of the *air,* so as to make it return to the state of *aqueous vapour;* but there never happens any chemical *decomposition,* without a *recomposition* of some other kind. Some *fluid* must necessarily be mixed with a part of the *atmospherical air,* in order to seize on the ingredients which had produced it from the *aqueous vapours;* but then, some new *combination* must certainly take place; and we have reason to suppose this to be the case, since the influence of the atmosphere on terrestrial phenomena is so various, that *rain* considered alone, is very far from explaining it. The immediate existence of such new combinations does not yet appear to us during the *simple rains,* but we are sure that they take place in the *tempestuous rains,* when we consider their concomitant effects. Different sorts of *fluids* may occasion the change of *air* into *aqueous vapour;* but the new combinations which then take place by the ingredients which it loses, are different, as well as the part of the atmosphere in which the operation happens; a circumstance that varies the appearances of the rains. Now we know from Geology, that at the period of the deluge, there happened very considerable changes, not only in the *atmosphere,* but in the *waters of the sea,* and with regard to the new continents, very different from the former, by being covered with strata of different sorts, which did not exist on them. It is therefore very possible, that the *tempestuous rains* are among the consequences of the *changes* which happened, at that epoch, on our globe, and that consequently the *rainbow* which appeared after the *flood,* might be a new phenomenon, connected with a state of the earth, in regard to which it became a real token of what GOD said to NOAH, at v. 15 of the same chap. "that the waters should no more become a flood to destroy all flesh;" or in the language of Geology, that the new continents were not exposed to sink below the level of the

the present sea, in consequence of internal operations, by which the state of the atmosphere could be still altered.

40. Admitting this idea, that the *rainbow* appeared for the first time to NOAH and his family, at the moment when GOD declared his *covenant* with the new inhabitants of the earth, let us represent to ourselves how they must have been struck with all its circumstances, by the novelty and magnificent appearance of the phenomenon, and by the divine revelation which presented it to their notice as the *token* of that covenant; considering that necessary impression, which, if the event was real, must have been felt by NOAH and his family, we shall find, in the proof of its having been produced, *that* of the reality of the event. On this subject it is sufficient to read that section of Mr. BRYANT's *Analysis of Ancient Mythology*, (II vol. p. 341) that has for its title, "of JUNO, IRIS, EROS, and THAMUR;" in which the learned and judicious author unfolds, what the ancient mythologies contain of allusion to this *sign*, as marking an era of the greatest importance in the history of mankind. The *Iris*, either as representing or accompanying *divine love*, and even expressly as the emblem of a *solemn covenant*, became an object of *worship* among the first people which separated from the family of *Shem*; and Mr. BRYANT gives a particular instance of this (p. 414 of the same vol.) in the design of a piece of sculpture, cut in the rock near the *Campus Magorum* in *Persia*, copied by *Thevenot*, in which *Eros*, that is to say, *divine love*, represented by a winged infant, is setting on a *rainbow*, near which is the figure of an *old man* in the act of *adoration*. Lastly, Mr. MAURICE, at p. 347 of his work, quotes also the mythology of the *Chinese*, where they make their great Deity FOHI, to spring from a *rainbow*; and this personage, in other respects, bears in their mythology all the characters and attributes of NOAH, as saved from the deluge. Here then are pointed traits of a general tradition relative to the *rainbow*, considered as a great *sign*; and to this, *natural history* and *Geology* add their evidence. Now, if we consider the simplicity and conciseness with which MOSES relates this circumstance, we shall find, that the Israelites must have also known it by tradition from their progenitors; and it is a fresh proof, that he relied on this tradition, through the whole of his narrative.

41. There

41. There is also a very remarkable circumstance, which refers at once both to the history of the *Flood*, and to the events that are related with respect to the *first man*; a connection that we have already noticed in the case of the plants which require cultivation to preserve them. In describing the *Garden of Eden*, with the view solely of giving to the Israelites, an account of the origin of evil, and of the remedy revealed by Divine wisdom, MOSES mentions the following circumstances, (Gen. ch. 11. vv. 10. &c.) "And a *river* went out of *Eden* to water the garden, and from thence it was parted and became into *four* heads: the name of the first is *Pison*;—the name of the second is *Gihon*;—the name of the third is *Hiddekel*, which goeth towards *Assyria*, and the fourth river is *Euphrates*." Here, I say, we have a description given by MOSES in the 2d chapter of GENESIS; and it could not but be present to his mind, when at the 6th chapter he began the history of the Deluge. The *Israelites*, to whom he addressed himself knew well an ASSYRIA and EUPHRATES, but these were very differently situated; no *river* existed in these countries that was divided into *four* heads, of which one was *Euphrates*, and another ran towards *Assyria*: how then could they have borne this apparent contradiction between the narrative of MOSES and facts, if for them it had been real?

42. We shall see here, from the very beginning of the book of GENESIS, what I have observed with regard to all the other parts to which I have had occasion to refer, that the divine source from which it is derived, appears more clearly in all those circumstances that, to those who have not studied them with attention, seem to be *improbabilities*. At ch. 2, where MOSES spoke of the *Garden of Eden*, he was describing a place which had belonged to the ancient continents; but when, in chap. 6, he came to the history of the Deluge, he began with the declaration of GOD to NOAH, that those continents should be destroyed. The Israelites, therefore, were not induced to make the critical remark to which I have just alluded; they knew that the names *Assyria* and *Euphrates* were *antediluvian*, and only transferred to the new continents, as we have since seen it practised universally by colonies, who name the new settlements in the countries which they adopt, after the correspondent names of their native countries. MOSES, in speaking of the Garden of Eden, described a *real* *place*;

place; for he could not be guilty of so gross a blunder as the contrary would imply; but this place existed no longer, since the continents on which it was situated had been *destroyed*: Geology even, in commenting on the narrative of MOSES, leads us to think that it was destroyed when ADAM and EVE, banished from the spot where they had transgressed the command of GOD, were compelled to apply themselves to agriculture for their subsistence: It is, I say, very probable, from the history of MOSES, that a *volcano* burst forth to bar the entrance of this original place of their abode, while it sunk into the sea. I have shown that *volcanic eruptions* make a part of the history of the ancient sea, and MOSES mentions a *flaming sword*, which guarded the entrance of the Garden: now in other parts of sacred writ it is said, GOD *maketh his ministers flames of fire.*

43. Lastly, after the account of the different circumstances of the Flood, and of the first establishment of NOAH and his family on the new continents, MOSES passes on to their posterity. He sets out with the first generations of the three sons of the great Patriarch, in order to mark the course they pursued in forming their first settlements, whence we are able to trace the origin of the traditions of the Deluge among the *Pagan* nations: after which, confining himself to the posterity of SHEM, which was his principal object, he first attributes to NOAH a length of life equal to what he had assigned to the *antediluvians*; he lived, he tells us, 950 years. From this time he represents the terms of man's life as decreasing; already SHEM lives only 600 years; nevertheless, in this course of successive decrease, ABRAHAM, the common Father of the Israelites, and on whom he never ceases to fix their thoughts, is yet spoken of as having attained to 175 years, after which the common term of human life was gradually shortened to its present duration. Here is the last *Geological* fact in the account of MOSES (the rest being only the history of the Hebrew nation), but this is well worthy of consideration.

44. The ancient mythologies of the Pagans also assign very long lives to their first chiefs; and after the account which I have given of the origin of their idolatry, it is natural to think, that the real facts relative to this object, had so raised their imaginations as to lead them to attribute *immortality* to these founders of the new race of mankind, and to augment in proportion

proportion the distance of time since they appeared on the earth. Every thing became gigantic in the ideas of these primitive people, when their traditions had become the only sources of their knowledge; because these contained real prodigies; and as in this strain they disfigured more and more the facts themselves, it is not surprising that their *chronologies* are at length become pure fictions.

45. If MOSES, as unbelievers suppose, had ransacked these sources, can we bring ourselves to believe he could have committed such a blunder as to drop this veil of *antiquity* in his Chronology, so as to make (in his address to the Israelites) but a few generations to have passed between NOAH and the times of ABRAHAM, their common father, at the same time describing the term of man's life as having decreased in this short period, from 950 to 175 years, with again a decrease of one half, from the time of ABRAHAM to that in which he wrote? No; it is not so that designing people invent. It must necessarily be allowed that MOSES relied on not being controverted; and we now see that he spoke nothing but the truth, since our continents (that unalterable *depôt of chronometers*), confirm his *chronology*.

46. Here again, some inattentive commentators on the book of GENESIS, have, by their systems, given weight to the arguments of unbelievers; bending the expressions of this great book like a *leaden rule*, and in that arbitrary manner of interpretation, either contradicting some ancient *astronomical monuments*, or depending too much on others. It is not necessary for me to enter into these details, because I shall again show, that the *literal* sense of GENESIS is the *truth:* but I must for a moment stop to consider a point of some importance on this head.

47. I have in view some chronological systems immediately deduced from those ancient astronomical monuments, under the idea, that nothing has happened with respect to the *movement* or *position* of the earth that is foreign to the *causes* or *laws* which serve as a basis to *astronomical* computations. This supposition is very natural on the part of *Astronomers;* but Geology does not permit us to assent to it, because the *revolution* which occasioned the deluge must have had some influence on our globe in this respect.

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If we first consider the *statical* consequences likely to have followed from a sudden displacing of such a mass as the sea, we must judge that the *velocity* and *direction* of the motion of the parts of this mass, which came to change its latitude, could not but have some influence on the *velocity* and *direction* of the motion of those parts of the globe to which they flowed, and on which they were detained, and thus occasion some change in its *rotatory* motion with respect to the *position* of its *poles*, or the *inclination* of its *axis* to the plane of its *orbit*. Moreover, the sinking down of the continents, together with a portion of the waters of the sea, having filled up vast caverns in the interior of the globe, there could not but follow some sensible alteration in its *centre of gravity*, and consequently in the direction of the *plum-line* on some parts of its surface. These changes doubtless could be but small; however they do not the less prevent true *chronological* inferences being drawn from some *astronomical traditions* among the Asiatics; since, in comparing *antediluvian* traditions with posterior observations, the difference between them may partly depend on these changes.

48. I have called *traditions* the *astronomical fragments* discovered among the people of Asia, because M. BAILLY has proved that these *fragments* had an origin prior to that of these people themselves: they being mixed with errors, which are the same in the north and the south of Asia, and into which the original observers could not have fallen. Now, from all that I have hitherto proved, these *traditions* could proceed only from NOAH; that is to say, from the first founder of the new race; and this we can also collect from the ancient *Mythologies*, where the great chieftain from whom they derived all human knowledge, is, among other things, represented as having instructed them in *Astronomy*. This likewise is the most probable reason why this science, not having its foundation in their own discoveries, but following the turn their fancies took in all other respects, degenerated into *Astrology*, the progress of which may be found in the works of Mr. BRYANT and Mr. MAURICE, the latter of whom may especially be consulted at p. 346.

49. I now return to the *long lives* both of the *antediluvians* and of the first generations of men after the Deluge, a circumstance for which it is not difficult to assign a physical

cause, since we have already had occasion to notice considerable and successive changes among all classes of organized beings, before and after the Deluge. These changes, the consequences of those which happened both in the interior and at the surface of the globe, were the effects of the same causes that have produced so many greater effects of different kinds, of which I have given an account in these Letters; and we may readily conceive from analogy, that the *long lives* of of the *antediluvians* were connected with the state of the earth; that the family of NOAH, bringing with them to the new continents a great strength of constitution, communicated it to their first descendants, and that it diminished only by degrees. All is connected together by the same causes, in the long series of *Geological* phenomena, (including those of the *organized beings*,) which I have described in this work; and it is the certainty and harmony of these phenomena, which, by irresistibly drawing aside the veil that covered real events in the ancient mythologies, and refuting the erroneous interpretations of GENESIS, finally evince that all the parts of this sublime cosmogony are derived from the fountain of TRUTH.

50. As for the final causes, of which we cannot doubt after so many proofs of the interference of GOD in the events of humanity, it is still easy to trace them in this account of the duration of the *life* of man. The first continents having been peopled only by the descendants of ADAM and EVE, and the present continents by the family of NOAH, we find accordingly a sublime agreement between the *long lives* of men in these two periods, and the rapidity as well of the population as of the advancement of the arts, of which we find the traces so simply exposed in the narrative of MOSES; for it would be easy to prove that this long term of *life* quadrupled at least every effect which the same length of time would have produced among men whose lives would have been only of the present duration. But when the human race was renewed on continents which the sea could no more overwhelm, and had so multiplied, that men would by degrees crowd together on the same parts of the earth, it was a sublime dispensation of the wisdom of the CREATOR, to shorten human life, since it shortened the dominion of the passions of individuals.

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51. In thus terminating the physical explanation of the *eleven first chapters of* GENESIS, containing the history of the earth, from the epoch, when *light* was first added to the other elements which compose it, to the time of the calling of ABRAHAM, I feel it incumbent on me to recapitulate the motives that have led me to these investigations. What can we determine with certainty respecting the *origin* and *nature* of *Man*, without knowing his *history*?—How can we know any thing of the *history of Man*, except we know sufficiently the *history of the planet* which he inhabits?—How can we learn the *history of this planet*, without studying the *monuments of its revolutions*, and all that Natural Philosophy can discover to us of their causes? Such are the questions which have induced me to devote near 50 years of my life to these studies, including the history of MAN himself; and as they have contributed more and more to impress on my mind a firm faith in our holy religion, I have found the reward of my labours in an inward satisfaction, which the vicissitudes of my life have never been able to shake.

52. GOD, by inviting us in his Revelation to study *Nature*, has laid a foundation for the re-establishment of the faith, when the lapse of time, and the caprices of fancy and passion, would have led the way to incredulity among mankind. *Faith* had been gradually established by *prodigies*, of which men had themselves been witnesses, and which they had transmitted to their successors; and now it is supported by proofs of the existence of the earliest and most important of these *prodigies*, which will serve gradually to dissipate the obscurities produced by the fabulous accounts of Nature, which men, who pretend to enlighten the world, have propagated. Then will mankind generally acknowledge a SUPREME LAWGIVER, who has given them *rules* and *instructions;* and they will in the end discover how much it concerns them to listen only to HIM.

I have now, Sir, finished that abstract of my Geological inquiries, and of their results, which you thought would be sufficient to enable those already versed in the study of these phenomena, to comprehend the whole of their connections, and to excite in others the desire of fixing their attention on so important a subject: but if it may answer your expectation

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in the first instance, I doubt of its success in the last. The objects of *Geology* are very numerous; they can interest only when they are so understood as to reflect light on each other; they will remain dark if not sufficiently explained, and then no interest can be produced. This, I fear, may have been the case with many readers of these extracts; but as they have the same interest as the learned to know what foundation our faith derives from Nature, I think it my duty to resume the same plan on a larger scale, in order to explain the whole in such a manner that every attentive person, of a common degree of information, may be so forwarded in Geology as to find readily new sources of knowledge in a multitude of common objects, which remain silent for those who want previous information. This I have undertaken in a work which I intend to publish.

I am, dear Sir,

With much Regard,

Your's, &c. &c.

DE LUC.

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